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BRAITHWAITE'S RETROSPECT.

VOL. LXXXV. JANUARY—JUNE, 1882.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY
W. BRAITHWAITE, M.D.
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AT THE LEEDS SCHOOL OF MEDICINE, ETC.

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AFFECTIONS OF THE SYSTEM GENERALLY.

Acute Rheumatism.—*The Salicylates.*—Acute rheumatism is a malarial fever; the rheumatic poison a malaria; and malaria consists of (and all recent research tends to confirm this view) minute organisms. The symptoms of acute rheumatism are the result of the action of this special organism on the system. The local joint and heart affection are the result of the action of these organisms on the fibrous textures of the joints and heart. The salicyl compounds produce their anti-rheumatic effects not in virtue of any special action which they exercise on the system, but solely in virtue of their destructive action on this organism: they arrest the rheumatic process not by acting directly on the affected tissues, but by destroying the poison which causes disturbance in these tissues. During the course of this action the physiological effects of the salicyl compounds—that is, the evidences of their action on the system—are frequently manifested; but these effects, so far from being looked upon as essential, are observed with regret; we do not wish them, and are very sorry when they appear; but if it were their full action on the system that we wanted, we would give them till we did produce these effects, and would give them with that object. That we get the full curative effects of this drug in the majority of cases without its physiological effects, is evidence that it is on the rheumatic poison that they act, and not on the system. This leads to the second point—their mode of administration. All evidence shows what, from the time that I first introduced this mode of treating acute rheumatism I have all along persistently maintained, that to get their full beneficial action the salicyl compounds must be given in full and frequently repeated doses. (Dr. T. J. Maclagan, p. 42).

In a series of 1200 cases of acute rheumatism, at Guy's Hospital, treated by salicylates Dr. Hood says:—A critical examination of these tables points out clearly and decidedly that patients taking salicylate lose their pains more quickly than those who do not take this remedy. Out of Dr. Fagge's cases, of 350, 288 lost their pains within the first nine days of treatment; in my own series of 350 cases treated in a similar manner, 247 patients lost their pain in the same period of time; whereas of 350 cases treated without salicylates, only 141 lost pain within the nine days. Is the effect stable? Apparently not, for on looking at the tables we shall see that the relapses among patients taking the remedy are vastly increased; and on further examining the average duration of stay in hospital, we find that patients taking this drug remain perceptibly longer under treatment. A scrutiny of my figures closely corroborates Dr. Fagge's statements that patients soon lose their pains, but are left feeble and exhausted after the use of this remedy. Endeavouring to estimate the effect of salicylate treatment upon cardiac complication I have divided my 1200 cases into three series again—350 without, 350 with salicylates, and the remaining 500 without. The construction of this table gave me no little difficulty, for, as Dr. Fagge justly observed, "Much obscurity attaches itself to the interpretation of the cardiac murmurs heard during the course of acute rheumatism." However, I have felt bound to enter all those cases in which the heart was noted as being affected at some period or other during the time the patient was under treatment. Do not understand by this that I have included cases in which the sounds were mentioned as being rough, prolonged, or the like, but those cases only in which a definite bruit existed. The presence of such bruit would in most cases be endorsed by the opinion of the physician in charge. We find that among the 350 patients treated by the salicylates 241 suffered from heart affection of some kind or other; among the 350 treated without salicylates 227 suffered from this complication; of the 500 without salicylates 273 were affected. The proportion between the two classes is much the same, but what little advantage there is does not appear to lie on the side of the salicylate treatment. With respect to the treatment of acute rheumatism, this complication of heart affection appears to me one of the most important points for consideration. Acute rheumatism is an expression the sum of which comprises certain known factors—to wit, pain, fever, often dangerously high, and a liability to mischief of heart. There are, doubtless, other points which, for the moment, lie without the scope of our present enquiry. But weigh these several factors one with the other; the preponderance of one is wellnigh overwhelming. Any remedy vaunted as a specific in acute rheumatism must show in marked degree its efficacy in controlling—I would rather say in preventing—heart disease. On this one count alone salicylate acid must be prepared to stand its trial, and must further submit to the most severe cross-examination at the hands of the profession. (Dr. Donald W. C. Hood, Lancet, Dec. 31.)

There is a common agreement that certain definite effects are produced by salicin in cases of acute rheumatism, and that these effects may be fairly anticipated to follow from its employment. Whether, however, the advantages of speedy reduction of pain and fever are absolute, or are countervailed by equally weighty disadvantages, is the point on which as yet it is impossible to say a decision has been arrived at We cannot conceal from ourselves that the advocates of salicin all seem determined to accept its good qualities, and to ignore its potentiality for evil; but with the evidence, and the deductions from evidence, afforded by Drs. Hilton Fagge and Dalton Wood are we, it is fair to inquire, justified in regarding the matter as other than *sub judice?* Honestly, we think not; and honestly, too, we believe that further and more complete and extended investigation—especially of the complications set up by it—on the effects produced by salicin are required to enable a reliable estimate of its character as a remedy to be formed. (Ed. Med. Press and Circular, p. 39.)

CANCER.—We believe that all the facts regarding malignant growths may be explained on the hypothesis of their local origin, as was some years ago so strenuously insisted upon by Campbell de Morgan; and the differences in degree and extent of generalisation depend as much upon the nature of the primary growth as they do upon the individual organism on which it is grafted. Taking, then, this doctrine as established, how can we explain the lamentably frequent failure of surgery to radically cure the disease? The reason why this happens is that the disease has *not* been extirpated wholly, and this through no lack of judgment or boldness on the surgeon's part. Tissues that appear healthy, even to the microscope, may conceivably be already under the mysterious influence of the *materies morbi* generated by the growth of the primary disease; and if this may seem too extravagant a statement, it is certainly true of tissues that appear normal to the unaided eye. In this way only can the facts of local recurrence and recurrence in the lymphatic glands be explained. The same applies to the visceral metastases. At the time of operating no indication at all may be given of the liver being infected, and yet how many cases of mammary cancer eventually present extensive secondary growths in this organ. How can this be explained, except on the view that even at the time when it appeared possible, and indeed in which it really was possible, to remove the primary disease, the liver was already infected, although a long interval—in some cases measured by years rather than months—might elapse before the latent germs manifest themselves in that organ. There is no need to apply the argument to all the other seats of secondary cancer, but it is not difficult to make the application, which seems to us to be a sound deduction from the pathology of these growths. (Ed. Lancet, p. 25.)

Pre-Cancerous Stage of Cancer, and Importance of Early Operations.—Twenty years ago my suggestion was, that all suspicious sores should be considered to be syphilitic, and treated internally by iodide of potassium, and locally by caustics, until the diagnosis became clear. More recently, I have often explained and enforced the doctrine of a pre-cancerous stage of cancer, in the hope that, by its aid, a better comprehension of the importance of adequate and early treatment might be obtained. According to this doctrine, in most cases of cancer of the penis, lip, tongue, skin, &c., there is a stage—often a long one—during which a condition of chronic inflammation only is present, and upon this the cancerous process becomes engrafted. I feel quite sure that the fact is so. Phimosis and the consequent balanitis lead to cancer of the penis; the soot-wart becomes cancer of the scrotum; the pipe-sore passes into cancer of the lip; and the syphilitic leucoma of the tongue, which has existed in a quiet state for years, at length, in more advanced life, takes on cancerous growth. The frequency with which old syphilitic sores become cancerous is very remarkable; on the tongue, in particular, cancer is almost always preceded by syphilis, and hence one of the commonest causes of error in diagnosis and procrastination in treatment. The surgeon diagnoses syphilis, the patient admits the charge, and iodide of potassium seems to do good; and thus months are allowed to slip by in a state of fools' paradise. The diagnosis, which was right at first, becomes in the end a fatal blunder, for the disease which was its subject has changed its nature. I repeat that it is not possible to exaggerate the clinical and social importance of this doctrine. A general acceptance of the belief that cancer usually has a pre-cancerous stage, and that this stage is the one in which operations ought to be performed, would save many hundreds of lives every year. It would lead to the excision of all portions of epithelial or epidermic structure which have passed into a suspicious condition. Instead of looking on whilst the fire smouldered, and waiting till it blazed up, we should stamp it out on the first suspicion. What is a man the worse if you have cut away a warty sore on his lip, and, when you come to put sections under the microscope, you will find no nested cells? If you have removed a painful, hard-based ulcer of the tongue, and with it perhaps an eighth part of the organ; and, when all is done, and the sore healed, a zealous pathological friend demonstrates to you that the ulcer is not cancerous, need your conscience be troubled? You have operated in the pre-cancerous stage, and you have probably effected a permanent cure of what would soon have become an incurable disease. I do not wish to offer any apology for carelessness, but I have not in this matter any fear of it. (Mr. J. Hutchinson, Brit. Med. Journal, Jan. 7.)

Enucleation of Scirrhous Glands in Axilla.—The following are Mr. Gould's conclusions upon the question of enucleating the axillary glands in cases of removal of scirrhous mamma:—1. Partial excision of a cancer should never be performed. 2. Where breast and axillary glands are cancerous the breast alone should never be excised. 3. That in very many cases of cancerous axillary glands excision can be performed with only slight additional danger, and with great benefit to the patient. 4. That the cases where such excision is not practicable are those in which the growth has spread to the chest walls. Pressure upon, or even adhesion to, the main bloodvessels and nerves of the axilla is not an insuperable bar to the operation, if sufficient care be taken. 5. That when no enlargement of axillary glands is to be made out, those glands may yet be infected with cancer, and that, therefore—6. Even when not evidently diseased, they should be removed at the same time as the diseased mamma, or, in other words, "it is advisable to remove the axillary glands *in all cases of cancer* of the breast submitted to operation." (Mr. Pearce Gould, p. 35.)

Chian Turpentine in Cancer.—It may be safely asserted that the effects of Chian turpentine in cancer of the uterus are tolerably uniform, and it is more than probable that the remedy effectually removes the cancerous infiltration surrounding the original growth, thereby preventing the extension of the disease. In treating a case of *Cancer of the Uterus or Rectum*, the following procedure is recommended for adoption. It may be premised that it is essential that the genuine drug only should be administered. It is a humiliating statement to make that even now some houses are supplying and dispensing other turpentines for Chian turpentine, and are even guaranteeing the genuineness of the article they supply. It is also necessary that the drug should be given in the form which is most convenient for assimilation. The essence of Chian turpentine, prepared by Southall and Barclay, of Birmingham, appears to me to be the most suitable preparation. This is prepared without the sulphur, and evidently does not contain any ether. The drug is in a state of minute subdivision, is easily digestible, suitable for all forms and situations of cancer, and is very palatable. One teaspoonful of the essence contains three grains of the turpentine. In place of the sulphur in the essence, Messrs. Southall prepare pills which contain sulphur, sulphate of copper, &c., and which they style, "pil. sulph. comp.," to distinguish them from the Chian turpentine pills. Two teaspoonfuls of the essence, with one or two of the compound sulphur pills, should be given three or four times a day, and after the medicine has been taken for about three months it should be omitted for about three days in every fortnight. The pills or mixture, prepared according to the original formula, may be given instead of the essence and sulphur pills. The vagina and rectum, even from the first, should be syringed daily with equal parts of vinegar and water. After allowing time for this to drain from the parts, it is advisable to insufflate into the vagina or rectum about ten grains of the following powder:—Tannic acid half an ounce, powdered charcoal two drachms, and powdered sulphate of copper ten grains. Mappin and Co., of Birmingham, make a vaginal insufflator by which the powder may be introduced into the rectum or vagina very efficiently. An insufflator may be improvised out of a vulcanite tube and a two-ounce india-rubber bottle connected with elastic tubing, but such an instrument is apt to become moistened by the secretions, and thus prevent the powder from becoming properly placed. The propriety of excising the os uteri in epithelioma of this part, as a preliminary procedure to the use of the Chian turpentine, may be questioned on reasonable grounds. The cancerous growth as it disappears under the Chian turpentine treatment leaves a shortening of the os uteri which brings the external rim of the os uteri into close proximity with the bladder and rectum, so that if the vaginal portion of the uterus has been removed, the subsequent contraction of the lower portion of the uterus drags on the rectum and bladder, causing great pain, with rectal and especially vesical troubles. In large pedunculated epithelioma of the os uteri the larger portion of it is perhaps best removed, taking care to leave the normal uterine tissue intact. In cases where the turpentine has been taken for some months, the dull curette may be used to remove the sloughing mass with advantage. When pain in the sacral or hypogastric regions comes on after the treatment has been pursued for two or three months, the use of morphia suppositories is indicated. To avoid the habitual use of opium, the tincture of Jamaica dogwood (Christy's), in one-drachm doses once or twice during the evening may be prescribed. Hemorrhage at the monthly periods is best met by giving the liquid extract of ergot in ten or fifteen-minim doses (which may be added to the essence of Chian turpentine), and to use locally the perchloride of iron in solution, or the dried persulphate of iron by means of insufflation. Diarrhœa and dysentery sometimes supervene during the treatment, for which the oil of eucalypus globules (in five-minim doses three times daily for a few days only) is a good remedy. This drug may be added to the essence of Chian turpentine. Anæmia consequent upon the occasional hemorrhages and serous discharges is a serious complication; much of the local treatment here recommended has been suggested to anticipate or prevent the anæmia. To combat this condition, Fellows' syrup of the hypophosphites, in one-drachm doses, may be given during meals, from the commencement of the treatment, in connexion with the Chian turpentine treatment (p.32.)

In *Cancer of the Vulva* Chian turpentine acts slowly, and as the disease frequently extends rapidly, an early excision of the growth, if possible, is necessary, the remedy being given for some time afterwards with a view to prevent a recurrence of the disease. (p. 33.)

The use of Chian turpentine in a large number of cases of *Cancer of the Breast* shows that it is a remedy of considerable power in relieving pain, of diminishing the size of the growth by causing the removal of the cancerous infiltration, leaving the more permanent fibrous stroma to be subsequently dealt with. In many instances the growth, although at first it was firm, and seemingly adherent to the chest walls, has become loose and easily movable from side to side, as if it were merely a foreign body, so that if an incision had been made in the skin it could have been pushed out. Several cases of primary scirrhus of the breast have been noticed in which, after the administration of the Chian turpentine for some months, the whole of the cancerous mass has sloughed away, appearing as a black gangrenous mass, leaving a cavity of considerable dimensions, the walls of which were composed of healthy tissue, and the wound healed with comparative rapidity by granulation. This process, however, is a slow and painful one, and suggests the propriety of anticipating the sloughing process by removing the growth with the knife after it has become sufficiently detached from the surrounding structures by the treatment. Several cases treated by this method have done well, the operation has been materially simplified, and, if the remedy is administered for some time after the operation, the experience gained shows that there is not so much likelihood of a recurrence of the disease. In recurrent cancer of the breast the remedy is often found to be of great benefit, and after it has been given for some weeks the application of the crystals of resorcin to the growth facilitates the disintegration of the mass. Resorcin is a powerful and at first a painless application. When its use is attended by much pain it may be mixed with equal portions of tannic acid and charcoal with good effect. Vaseline or chrisma dressing is all that is required after the application of the powder. (p. 33.)

In *Epithelioma of the Face* and other parts of the cutaneous surface the use of Chian turpentine in conjunction with the powder previously advised for the local treatment of cancer of the uterus, to which three drachms of resorcin have been added, very good results have been obtained. In two cases of *Cancer of the Stomach* very beneficial results have accrued from the special treatment. In cancer of the mouth and tongue the results have not been so good, in consequence of the rapidity with which the neighbouring glands became involved in the cancerous disease. (Prof. John Clay, p. 30).

DISINFECTANTS.—*Is Iodine a Disinfectant?*—The only real disinfectants are those which hasten putrefaction, as sulphurous acid, bisulphide of carbon, all gases that contain hydrocarbon, such as coal gas, and others of too dangerous a character to be of any practical value. These substances may be advantageously divided into three distinct classes—namely, *disinfectants*, that is, those substances that destroy contagion by hastening the death and putrefaction of the living germs; *antiseptics*, those that decompose septic or putrid animal matter by converting it into carbon, &c.; and *deodorisers*, or those that decompose the effluvia of decaying vegetable matter. I am not aware that Dr. Richardson ever recommended iodine as a disinfectant proper, and some years ago he gave me the credit in a contemporary journal of being the first to recommend its use as an antiseptic. Septic poison, as it occurs in the so-called cases of puerperal fever, may be and is immediately destroyed by the accoucheur when unfortunately he has had to deal with a case of the kind, by his going into a small apartment, placing a few scales of iodine on a saucer, and holding beneath it a spirit-lamp for a few seconds, when he will be surrounded by the beautiful violet-coloured fumes of iodine, and may then go without dread or fear and attend any other female in her confinement, the septic poison having been converted into harmless carbon. (Dr. A. Wynn Williams, Lancet, Feb. 18, p. 290.)

GOUT, AND LEAD-IMPREGNATION.—The connection between lead-poisoning and gout is well established, although the nature of that connection is still but little understood. It is not too much to assert at once that the facts adduced on this subject by Dr. Garrod, first in 1854, and subsequently added to by him, have been fully confirmed by other observers, and the merit of fully setting forth the connection between the two disorders rests with him, although previous indications of it were made known more than a century ago. Dr. Garrod states that "at least one in four of the gouty patients who had come under his care in hospital had at some period of their lives been affected with lead, and for the most part followed the occupation of plumbers or painters." He has demonstrated that lead distinctly diminishes the secreting powers of the kidneys for uric acid, and Charcot likens this inhibitory action of the metal to paralysis of the kidney. The uric acid is consequently retained in the body. Due regard being had to these facts, it becomes easy to see a very close relation between lead-impregnation and the frequent occurrence of gouty manifestations. (Dr. Garrod, Dr. Duckworth, p. 45.)

MALIGNANT TUMOURS.—*Electrolytic Treatment.*—Prof. Semmola of the University of Naples had intended to read a paper on the use of electricity in the local treatment of malignant tumours at the International Medical Congress. He was unable, however, to attend, but the subject, although not new, possesses so much interest that the facts he intended to record are worthy of notice. His experience was gained in the treatment of six cases—one of epithelium of the right breast, the size of an orange; a fibro-sarcoma of the right breast; two cases of sarcoma of the right breast; one case of sarcoma of the left breast; and one cysto-sarcomatous tumour, growing from the upper third of the arm. In five of the cases amputation of the diseased part had been recommended by experienced surgeons, and the sixth was a case of recurrence eighteen months after the removal of the primary sarcomatous tumour. The tumours are said to have had all the clinical characters of malignant growths, and to have been examined microscopically by Prof. Petrone. The needles employed were the steel needles in common use for electrolytic purposes, and they were passed deeply into the tumour, converging towards its centre. In his earlier experiments only the negative pole was thus inserted, the positive pole being placed on the chest, but in the later ones he found it beneficial to pass in both poles of the battery. The batteries used were Stöhrer's and Onimus's; with the former the deviation of the galvanometer was 90°; with the later 60° to 75°. In small tumours one inserted needle was found sufficient; but Dr. Semmola belives that he has obtained a more settled action. As a rule, passing the needle causes next to no pain or difficulty, but at times small sclerotic foci interfere with their transit. Very rarely did any painful inflammation attack the spots of puncture. The constant current should be used frequently, even three times in the twenty-four hours, and allowed to flow through the new growth for an hour each time. A weak current, long-continued, seemed to be better in its effects than a stronger current acting only for a short interval, and it is stated that the former has a greater modifying effect upon the local chemistry of nutrition. While the current is passing the galvanometer oscillates between 10° and 15°. This shows that there is some modification in the resistance offered by the tissues, and is a mark of the change produced by the current. In one case the treatment was ended in twenty-four sittings, but in another it extended over seven months. In the case of cystic sarcoma, after two applications of electricity, inflammation and destructive suppuration set in. In none of the cases were the lymphatic glands affected. Dr. Semmola suggests that electrolysis cures malignant tumours in one of three ways: by producing small foci of inflammation with consecutive sclerosis, the tumour being converted into a small, indurated and harmless lump; by producing a colloid and fatty degeneration, especially in tumours with this tendency; and by exciting destructive inflammation and suppuration of the tumour. Along with this local treatment in all his cases Dr. Semmola has combined the administration of large doses of iodide of potassium, with the view of gravely modifying the general nutrition. (Prof. Semmola, Lancet, Nov. 26, p. 921.)

SEPTICÆMIA.—It has been found that a septicæmic virus, which has been exposed to a temperature sufficient to destroy all the bacteria it contains, will still cause the death of an animal into which it is injected. It is currently assumed that the animal is poisoned, not infected—*i.e.*, that the virus causes death by its presence, not by its multiplication, and that a second animal cannot be killed by the injection of a small quantity of the blood or serum of the first, any more than in a parallel case of strychnine poisoning. To decide this question, septic blood and serum were heated, filtered, evaporated, and then injected. The animals died with all the symptoms and pathological appearances of septicæmia, just as if the uncooked virus had been used. Moreover, it was found that the blood-serum of the tissues and cavities contained the same micro-organisms and in the same quantity as animals into which the uncooked virus had been injected. The effect of heat was, however, to lessen greatly the virulence of the poison, so that a cubic centimeter was needed of a virus of which before the minutest quantity sufficed. Further experiments showed that septicæmia could be produced in other animals by the injection of even extremely small quantities of the blood- or tissue-serum of those that had been killed by the inoculation with the cooked virus, which appeared thus to have acquired by renewed development the intensity of which it had been deprived by heat. (Ed. Lancet, p. 10.)

Typhoid Fever.—*Carbolic Acid.*—I have found carbolic acid a most powerful therapeutical agent in typhoid fever. I have now employed it in about twenty cases with two deaths. It is most applicable to those cases of the fever where death is threatened from the "putrid state" of the old writers, when in the course of the second week there is nocturnal delirium, copious diarrhœa, depression, stupor, with sordes on the teeth and gums, and dry brown tongue, muscular tremors, and bronchial catarrh. I have found the acid most useful. It must be employed in large doses, freely diluted (ʒ ss.—ʒ j. daily in ordinary drink or abundance of water). It evidently acts as an antiseptic, and also as a nervine tonic, accelerating the healing and cicatrisation of the intestinal ulcers. Small doses, perhaps, in consequence of the nervous stupor which prevails in enteric fever are of little or no therapeutic value. (Dr. F. W. Warren, Medical Press and Circular, Jan. 11, p. 22.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Acute Traumatic Tetanus.—*Chloral and Bromide.*—I began by giving the patient steadily, chloral in ten grain and bromide of potassium in fifteen-grain doses, every two hours, increasing the frequency of the dose as the spasms were more frequent or severe. In addition to this I usually gave from five to ten grains of chloral hypodermically whenever I called to see him, which, during the early part of the time, was three or four times in the twenty-four hours. These hypodermic doses quieted him much more markedly than those taken by the mouth, and the influence of the drug remained for a longer period. About three weeks from the commencement, he lay down in bed for the first time and opened his mouth better. (M. J. H. Salter, p. 67.)

ADULT SPINAL PALSY.—As to the treatment of these cases, which after all is the most important matter for the patient, the following seem to me to be the most rational principles to act upon, and were kept in view in those that have come under my care:—In the acute stage, a stage having probably as its anatomical character a condition of hyperæmia of the cord, and marked by exalted sensibility; fibrillary tremors; and sometimes twitchings of muscles; disturbed areas of sensation, as if, for example, a hot metal plate were held close to some portion of the skin; a feeling of the most intense fatigue of limb and back on undertaking even a slight exertion (the natural healthy colour of the face being all the time but little, if at all changed); means calculated to diminish hyperæmia are most useful, and of these means the internal administration of ergot and the employment of counter-irritation, such as that by blisters or Charcot's white hot cautery along the spine are perhaps the best. But so soon as this stage has passed, and paralysis and wasting have set in, means having for their object to nutrify the nervous centres and the muscles should be adopted. Of these means, strychnia by mouth and hypodermially injected, phosphorus, Faradism and shampooing of the muscles, and the constant current to the cord, are perhaps the best yet known, and the persevering employment of them will be often followed by immense benefit, even in cases of long standing and great severity. (Dr. W. Carter, p. 59.)

NITRITE OF AMYL.—*Therapeutic Uses.*—Dr. Edgar Kurz, of Florence, has found this medicament so useful in the various aches and pains of every-day life that he has persuaded many families of his acquaintance to keep it on hand as a domestic remedy. It is an excellent external application for stomach-ache, colic, toothache (whether nervous or arising from caries), neuralgia of the trigeminus, of the cervico-brachial plexus, &c. It is superior to anything else when inhaled in so-called angio-spastic hemicrania, giving rapid relief in the individual paroxysms and prolonging the intervals between them. No trial was made in cases of angio-paralytic hemicrania, since in this affection the drug would be physiologically contra-indicated. It has a very good effect in dysmenorrhœa, especially when occurring in chlorotic girls; in mild cases external applications suffice, otherwise the drug should be inhaled (when complicated with inflammatory conditions of the uterus or appendages the results were doubtful or negative). It was found to be of much value in attacks of dizziness and faintness occurring in anæmic individuals, as also in a fainting-fit from renal colic, and in several cases of collapse during anæsthesia by chloroform. It has been recommended in asphyxia from drowning, hanging, and in asphyxia of the new-born. But the first indication in these cases is the induction of artificial respiration, after the successful initiation of which inhalations of nitrite of amyl doubtless assist in overcoming the concomitant spasm of the smaller arteries. One of the most important indications for the use of the drug is threatening paralysis of the heart from insufficient compensation. In such cases it is necessary to gain time until digitalis and alcoholics can unfold their action, and here nitrite of amyl stands pre-eminent. (Dr. E. Kurz, Practitioner, Feb., p. 139.)

In a recent work on the *Sympathetic Nerve*, an interesting series of experiments with Amyl Nitrite is reported. It having been suggested that, if sleep is dependent on cerebral anæmia, amyl nitrite, which promotes the circulation in the brain, should awaken sleeping persons, Dr. Chapin, with the aid of a nurse, carefully applied a few drops of the drug to the nostrils of ten or twelve patients who were sound asleep, and in every case they awoke in less than one, or at most in two minutes. This was repeated on several evenings and on different patients, but with uniform results. Lest it might be the odour of the drug, or the irritation of the fifth nerve, or his presence near the bed, he tried on other occasions bisulphide of carbon and oil of peppermint, but succeeded in awakening not a third of those on whom they were employed. (Dr. C. O. Chapin, New Remedies, Aug. 1881.)

PATELLA REFLEX.—There are few symptoms of greater importance in the diagnosis of diseases of the nervous system than the patellar-reflex phenomenon, especially when it exhibits deviations from its normal condition. Supposing one of the hospital pupils is sitting with his right leg crossed over the left, and bent at an angle slightly greater than a right angle, when, with this stethoscope, which has an india-rubber ring round the pectoral piece, I strike the right leg just below the patella, immediately the foot is jerked forwards. Such is the phenomenon, and such is the ordinary method of testing for its presence. The following plan is suggested by Buzzard if it is desired to ascertain the mere presence of the "patellar-reflex:"—"The patient, who is seated, plants his foot firmly down at such a distance that the leg forms a little more than a right angle with the thigh. Whilst the observer rests the palm of his left hand upon the patient's thigh, he strikes with some implement held in the right hand several blows upon the ligamentum patellæ at about an eighth of an inch or so below the knee-cap. The quadriceps muscle can be felt, and (if the patient is in masculine garments) can be seen to contract more or less vigorously in response." The side of the hand can be employed as a percussor. If no response follows the blow the leg should be bared, and the observer should test most carefully by striking in the usual place, or on the patella, or the tendon above the patella, or the muscle itself, before assuming that the reflex is absent. In any disease in which the reflex nervous arc, at the situation mentioned, is injured, in either its sensory or motor portion the patellar-reflex is lost. This is exemplified in tabes dorsalis and in poliomyelitis anterior acuta. In fifty cases examined by Erb the patellar-reflex was absent in all but one, and he thinks that it is probably always an early symptom. In his experience it is never absent in young or middle-aged people in health, and hence he thinks that its absence may be regarded as almost pathognomonic of tabes in its earlier diagnosis. The lesion in tabes dorsalis is sclerosis of the posterior columns, or, to state the matter more correctly, of that portion of these columns known as the "posterior root-zones" (Charcot), involving the posterior roots on their way to the posterior cornua. This lesion causes an interruption of the sensory impulses in their passage to the spinal centre, and so the patellar-reflex is lost. Even without our knowledge of the morbid anatomy of tabes, we would almost be warranted in concluding that the motor portion of the reflex arc was intact, from the fact that good voluntary movement of the lower limbs is preserved, and absence of any wasting of the muscles; while, on the other hand, that the sensory portion of the loop is affected, is apparent from the lightning pains and the anæsthesia. (Dr. J. W. Byers, p. 52).

SCIATICA.—In nineteen cases out of twenty in which the diagnosis of sciatica is suggested there is no affection of the sciatic nerve whatever. They are simply cases of arthritic disease of the hip in one or other of its various forms—acute gout, chronic gout, rheumatic gout, subacute rheumatism, or chronic senile rheumatism. Both by the public and the profession these cases are constantly called sciatica. Our workhouse infirmaries are full of chronic cases under that name, and I speak advisedly when I say that I feel sure that they are almost all examples of *morbus coxæ senilis*. Of the cases of sciatica which are not hip-joint rheumatism, some are probably affections of the fascia or periosteum near to the hip; a minority are possibly affections of the sciatic nerve itself.

In these latter it is the sheath of the nerve which becomes painful. The pain may be darting or may radiate, but it does not pass down the nerve-tubules or in any way make the patient conscious of their course. The diagnosis of true sciatica is to be based upon the discovery of tenderness restricted to the trunk of the nerve, and involving a considerable part of its course. Examples of this are decidedly rare, and their recognition without risk of error is a matter of great difficulty. (Mr. J. Hutchinson, Medical Times and Gazette, Jan. 28, p. 85.)

SEA-SICKNESS.—Pressure over the abdomen is by far the most hopeful remedy we possess. A well-made mechanical appliance, specially designed for this purpose, and made for me by Messrs. Maw, Son, and Thompson, Aldersgate Street, I have found to be most efficient. This support fixes the abdominal organs, it also helps to do the work of the abdominal muscles until they become sufficiently educated to reflexly appreciate the exact moment when their duties should be performed. Bearing upon this latter point, it is well known that old sailors, after a long Pacific voyage, where the seas are regular and very large, frequently become sea-sick when crossing the British Channel and the Friths, where the waves are short and choppy, and the movements of the vessel very different from that which they have been accustomed to. In this case the abdominal muscles of the sailors have been well accustomed to one kind of movement, but are utterly unable to appreciate the other. It is found by experience that if the abdominal muscles can be kept in an almost continuous state of contraction, the tendency to sea-sickness is very much lessened. We can do this either by strongly willing to make these muscles contract, or by making a continuous expiratory effort as in singing or whistling; but most will agree with me that this would be by far too tedious a procedure to recommend. It is well known that certain positions in a vessel are more likely to provoke sea-sickness than others. For example, an upright position with the back to the bows of a ship, and at one or other extremity of the vessel, is more likely to induce sea-sickness than the horizontal in the centre of the vessel would be. If from any cause the voyager is unable to assume the recumbent position, the next best one is to sit down on a low chair or stool, with the knees drawn up to the body, the body being bent forwards, and a roll of clothing pressed between the stomach and the knees. I have often noticed that this position gives relief, the extreme pallor of the face changing to a normal colour. The abdominal support before alluded to is in the form of a belt with five air-cells worked between the two layers of the material—three in front and two behind. These air-cells are all connected together by small air-tight tubes, and with the inflating nozzle and outlet plug. The belt is applied to the body over the jersey, and buckled comfortably tight. When the sea is gentle and smooth there is no need to tighten the belt by inflating the air-cells, but if rough these are inflated until such compression is produced as will convey a sense of support and comfort. The belt when inflated is not noticed under a loose waistcoat. If worn by ladies it should be worn without stays. The pads are applied over the small of the back on either side of the spine. When the air-cells are strongly inflated, the belt is capable of sustaining a person in the water. When used for this purpose, care should be taken to secure the shoulder-straps crossways over the neck, and the belt should be drawn up under the arms. (Mr. P. W. G. Nunn, Lancet, Dec. 17, p. 1038.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

EPISTAXIS.—*Immediate Arrest of.*—Many instruments have been made to stop bleeding from the nose, one of which is described as follows:—"It consists of a gum-elastic tube about five inches long, with lateral perforations near the end, and covered with thin caoutchouc membrane in the form of a spirally twisted bag for the last three or four inches of its length. To use it, the membranous bag is smoothly folded over the continued tube, and the whole being oiled (diluted glycerine is better) is passed along the floor of the nares till it reaches the pharynx. The bag is now inflated, and if a stop-cock is fitted, the air is kept in by turning it as soon as sufficient tension is obtained." The cavity of the twisted bag could be injected with water, if it were desired; but I have never found this necessary. When I recollect what "bleeding from the nose" was in old days, I cannot be too thankful to Dr. Rose for his simple and effective invention. In an obstinate accident of this kind the victory is half won when a man is armed with an apparatus which he knows is sure to succeed; and I am now speaking of cases in which he wishes to succeed, and which are not forms of natural blood-letting to be encouraged. I can recommend this instrument for facility of introduction; the extent and evenness of the inflated area; and the possibility of its remaining *in situ* for 36 or 48 hours, when it may be gently removed, and the hemorrhagic nostril syringed with some cold astringent fluid for the washing away of blood *débris.* (Dr. Spender, p. 77.)

The Inflating Nasal Plug.—Mr. Howard's instrument is perhaps an improvement on the one above described. He says:—Mine consists of an India-rubber bag, of the length of the nose, compressed in the middle, so that the two enlarged ends plug respectively the anterior and posterior nares. The narrowing in the middle is made for the purpose of allowing the pressure to be as little as possible on the inferior turbinated bone. It is passed into the nose by a probe hitched on the under side. To the anterior extremity is attached the tube, with stop-cock, by which it is inflated. (I have sent one to Dr. Spender, as I gather from his article that he has not seen it.) I claim the following advantages for my instrument. It causes less pain when *in situ*, on account of the greatest pressure being at the ends, and not in the middle, on the inferior turbinated bone. It cannot be pulled out or blown out while sneezing (as, I believe, Dr. Rose's can be). After insertion, and having been filled with water, it holds more water by about one-fourth, proving that the cavity of the nose is more occupied, especially at the ends, where the pressure is needed, and therefore better plugged. I have the authority of Mr. T. Holmes for saying that my invention is superior in construction to any he has seen. It is made by Walters and Co., of Lambeth Palace Road, and is called the "Inflating Nasal Plug." (Mr. H. C. Howard, p. 78.)

EXTERNAL ANEURISM.—*Compression by the Elastic Bandage.*—In 1875 Staff-Surgeon Walter Reid, R.N., operated on a case of necrosis of the femur, and during the whole period of the operation, which was protracted to over an hour, the limb was kept quite anæmic by means of Esmarch's elastic tourniquet. Having a case of popliteal aneurism in the hospital at the time, it occurred to Dr. Reid that this same tourniquet would be a ready and safe means of keeping the sac full of stagnant blood for a time sufficient for its coagulation. Accordingly, next day, at 10.20 a.m., he "bandaged the limb with the elastic roller, from the toes upwards to the junction of the middle with the lower third of the thigh. It was applied lightly over the aneurism, so as not to compress it. The elastic tubing was then wound round the limb over the highest turn of the bandage, which was now removed. The entire circulation below the tubing was found to be arrested. The limb had assumed a death-like pallor, and gradually lost temperature. The aneurism was of its usual size, and pulseless. After fifty minutes, the patient complaining of severe pain above the seat of constriction, a Carte's compressor was adjusted to the main trunk at the pelvic brim, and the elastic tubing removed." The aneurism was now found to be pulseless, and several small vessels round the knee-joint were felt beating strongly. Carte's compressor was applied lightly and intermittently, so as not to distress the patient, till the next evening, when all treatment was suspended, and the aneurism was cured. Dr. Reid remarks: "The explanation appears to be—the coagulation of the blood in the sac, consequent upon its complete stagnation. The aneurism itself probably remained full, but the entire circulation in the parts was arrested. My reason for compressing the main trunk for sometime afterwards was that the force of the blood-current might have broken down and washed away the newly-formed clot before it had time to become tough and solid, and the sac to contract over it." From these quotations it is demonstrated that Dr. Reid clearly appreciated the principles involved in the practice he initiated, and those who have followed him have not been able to make any important additions to the details to be observed to insure success. This first case was so strikingly successful that the method was soon employed in other cases by other surgeons, but it is not until quite recently that continental surgeons have availed themselves of this latest development of the plan of treating aneurisms by compression. Mr. Gould has collected more than seventy cases of external aneurism treated by Esmarch's bandage, which have been recorded more or less fully. (Mr. A. Pearce Gould, p. 169.)

HEART DISEASE.—*Nitro-Glycerine.*—This is a most potent remedy, and deserves to rank only second to digitalis in the treatment of disease of the heart. The form most generally used is a one-per-cent. solution in spirits of wine: one minim is the usual dose to commence with, but in some cases even less may be given with advantage. It can either be taken in water or one drop may be placed upon the tongue. The solution is almost tasteless, but within three minutes of being taken it begins to exert its peculiar physiological properties. It paralyses the vaso-motor nerves, and so dilates the blood-vessels; the face flushes, the temples throb, the pulse becomes dicrotic and much quickened; in some cases the head aches most violently, but in others only a sense of fulness and pain across the forehead is experienced, which lessens with each recurring dose, until ultimately no unpleasant effect, but simply a warming sensation all over the body, is produced. A feeling of nausea, or even sickness, is often caused by the earlier doses. The quantity may be gradually increased until 15 or 20 minims every four hours are given, but I have never found it necessary to administer such heroic doses. It is never wise to give more than one minim at first, for even this small quantity has produced most serious symptoms in certain individuals. The patient has fainted, and has become almost collapsed, but I am not aware that it has ever been followed by a fatal result. While useful in almost all cases of heart disease, I believe those in which it will be found of the greatest benefit are, 1st, angina pectoris; and 2nd, weak, dilated, and fatty heart. In angina it prevents an attack by keeping the blood-vessels in a constantly dilated condition, and thus prevents the backward pressure of blood upon the heart, which is probably the cause of the agonising pain of angina. In weak dilated hearts it gives relief by reducing arterial tension and thus lessening the amount of work the heart has to do; the heart, consequently, gains in power by the rest so given to it. As a rule, digitalis does not agree in these cases; but if thought necessary, it may be given with increased advantage in conjunction with this drug. In several cases of dilated heart, with small, weak, quick pulse, I have seen the beats not only increased in power, but much reduced in frequency, after taking nitro-glycerine for a few days, thus plainly showing that the heart had been relieved of much of its embarrassment, and as a consequence had gained in power. I have used this drug largely during more than two years, and each week my appreciation of its value as a remedy for this class of cases increases. (Mr. W. E. Green, p. 74.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ACUTE BRONCHITIS.—*Linimentum Crotonis.*—As soon as the patient comes under treatment, the chest, in whole or in part, is to be rubbed with two or three drachms of lin. crotonis. If the patient is seen at the very earliest stage, when there is only a slight roughness and pain behind the sternum, then it will be enough to have it rubbed into the episternal hollow, along the clavicles, and down the front of the sternum as far as the ensiform cartilage. Otherwise the whole front of the chest, the sides thereof under the armpits, and the back between the shoulders, should be rubbed. The best manner of application is by means of a tag of cotton-wool saturated with the liniment and rubbed till it is dried up. Care should be taken, and the patient warned, not to let the liniment *run* down the loins and abdomen, which it is apt to do. Also the applying hand should be carefully washed *immediately* afterwards. The application *at once* relieves the patient, and this relief is maintained and increased by having the surface rubbed covered over with a thin layer of absorbent cotton-wool, properly retained to prevent shifting. For this purpose an old chamois leather vest answers well. From repeated personal experience I know that the liniment applied in this way does not produce pain. Sometimes, after twenty-four hours or so, there is a feeling of tenderness or soreness; but, if the cotton-wool be not disturbed, and violent rubbing be not resorted to by the patient, this soreness is evanescent, and succeeded by an itching—sometimes very considerable—which remains for three or four days. It may be relieved by smearing the eruption over with fresh butter, lard, or vaseline *under the cotton-wool*, care being taken, however, not to disturb the cotton where it has become adherent with discharge. (Dr. R. Park, p. 92.)

ANTISEPTIC TREATMENT OF LUNG DISEASES.—This may be attempted by (1) causing the fumes of an antiseptic to permeate the apartment in which the patient is located; or (2) inducing him to wear a respirator or inhaler which admits of being charged with the antiseptic fluid. I have experienced disappointment in my use of the first, at least when used alone. Regarding the second method, I have endeavoured to carry it out by means of the naso-oral respirator or inhaler, which covers both the mouth and nose, and, on account of its valvular arrangements, can be worn for lengthened periods without inconveniencing the patients. In acute phthisis, the lungs can scarcely be acted on too freely or continuously by the antiseptic; and, as the nasal channels form the physiological respiratory highway, they, as well as the mouth, should be under strict antiseptic guardianship. In fact, I believe the necessity for antiseptic nasal respiration in phthisis to be the more important; and, from this point of view, I am at present experimenting with nasal respirators. In reference to the antiseptic employed, almost any volatile one may be used. Carbolic acid, after inhalation for some time, may be found in the urine in very minute traces, according to my friend Mr. Falconer King, who has made for me several very careful investigations on this point. It has never, in my experience, produced toxic symptoms, although used pretty continuously for several months, and in concentrated solutions. Under similar conditions, no traces of creasote can be found in the urine. A medical friend has lately recommended to me sanitas oil, as likely to prove an efficient substitute for carbolic acid and creasote in cases where the use of the latter is objected to. Dry inhalations should be used with caution in cases with laryngeal complications. My experience on this point agrees with that of Dr. Coghill, who found these applications very apt to aggravate the laryngeal mischief. This may be obviated by using a weaker solution of the antiseptic—say, one of carbolic acid or creasote to three of rectified spirit—and by freely applying to the larynx, three or four times daily, a solution of carbolic acid, morphia, and glycerine, by means of the laryngeal spray, which brings the fluid into direct and plentiful contact with the larynx. Not only has this a most soothing effect on the throat, but, at the same time, it seems to retard the progress of the laryngeal disease. My respirator is made by Mr. Gardner, South Bridge, Edinburgh. (Dr. G. H. Mackenzie, Brit. Med. Journal, Nov. 19.)

My attention was first called to this method of treatment last autumn by a patient of mine suffering from phthisis, who had found great relief from the inhalation of a solution of carbolic acid through an inhaler, recommended to him by Dr. von Messing, of Meran, in the Southern Tyrol. The inhaler is made of German silver, and is shaped like an ordinary respirator, but has a cavity enclosed between the outer surface and an inner plate; both plates are perforated, and the outer one can be raised at pleasure for the purpose of placing in the cavity cotton-wool saturated with the drug required for inhalation. The instrument is made by W. von Pernwerth, of Meran; it appèars to resemble almost exactly the respirator recommended by Dr. Coghill (see *Retrospect*, vol. 84, p. 105). Dr. von Messing very kindly sent me one; and in his letter, dated Sept. 6th, 1880, says, "I have heard of your desire to try the inhaler I am using for my patients, and have ordered one to be sent to you. I have been using it for about five years, and I am perfectly satisfied with the results. I generally use a mixture of 20 parts of carbolic acid to 100 of water, to be well shaken, and twenty to thirty drops poured out on the cottòn. I advise my patients to inhale through the apparatus, and exhale through the nose, three to four times a day, for ten minutes at first, and then for an hour; in this last case, wetting the cotton two or three times. In cases of bronchitis, it affords great relief and shortens the process; also in cases of hooping-cough. In consumption, it lessens the fever, by absorption of the carbolic acid, and facilitates the expectoration. In chronic bronchitis with bronchiectasiæ, it does away with the smell, and disinfects the air-passages. Violent spasmodic cough, above all at night, is immediately checked by the addition of 15 to 20 drops of chloroform." Since receiving this letter, I have tried the inhaler with very satisfactory results. In my last case, a case of phthisical consolidation involving a considerable portion of the left lung, and attended with hectic and purulent expectoration, the weight increased in one month from 7 st. 1 lb. to 8 st. 1 lb., during the use of the antiseptic inhalation; the fever entirely disappeared, and the cough and expectoration almost entirely. Now, unfortunately, a fresh patch of inflammation has occurred, which has brought back the fever and cough, but the temporary benefit was well worth having. In one case, I substituted the oil of Pinus Sylvestris for the carbolic acid with very good result, and should be inclined to try it again in cases of obstinate chronic bronchitis. (Dr. G. H. Batterbury, British Medical Journal, Oct. 22.)

Inhalation of Terebene in Phthisis.—For upwards of four years, antiseptic inhalation has been one of the principal remedies employed by me in the treatment of phthisis and certain other lung-diseases. The vapours of creasote, carbolic acid, iodine, and other antiseptics, have been used in more than four hundred cases, occurring both in hospital and in private practice. During the past two and a half years I have adopted terebene as a remedy in preference to the antiseptics mentioned above. I have used it in upwards of two hundred cases of consumption, bronchiectasis, chronic bronchitis, and other pulmonary complaints characterised by profuse purulent expectoration. It is very valuable, and its vapour possesses antiseptic qualities equal to any of the others. It has also a sedative action, allaying the cough and irritation, and is almost invariably preferred by the patients themselves to any of the other antiseptic inhalations, on account of its agreeable and fragrant pine-wood odour. In cases where the expectoration is copious and fœtid it is advisable to administer it internally in addition to the inhalation of its vapour. Given in doses of five minims, either in emulsion or with carbonate of magnesia, it acts as an aromatic expectorant. The researches of Cohnheim and others have shown how liable the lymph follicles of the intestine are to tubercular infection during the progress of pulmonary consumption, in consequence of the sputa being swallowed by the patient. Administered internally, terebene destroys the virus and lessens the risk of intestinal infection. Professor Maclean states that in dysentery terebene may be given internally several times in the day, without causing any irritation, as a corrective of the putrid secretion from the diseased intestine. In cases of tubercular ulceration of the bowels it sometimes lessens the diarrhœa, but I have found that a mixture containing bismuth and hypophosphite of lime gives greater relief in that complaint than any other remedy which I have employed. (Dr. McAldowie, *Ibid.*)

Skeleton Respirator for Antiseptics.—The respirator I have found best for antiseptic purposes may be called a "skeleton respirator." It is made simply and cheaply. It consists of an oval of wire measuring four inches and a half in its long diameter, and one and three-quarter inches in short diameter. This is bowed up one and a quarter in the middle, and fits round the mouth, one wire going over the upper lip, the other under the lower lip; the ends of the oval rest on the cheeks. From the wires of the oval as they pass the corners of the mouth four wires spring out, two at each side of the mouth, one being above and the other below. These wires are an inch long, and support a shallow cage capable of holding a double fold of lint measuring two and a half inches by one inch. The cage is formed of two light wire screens closing with a spring, the wires of the front screen being perpendicular, and those of the posterior screen horizontal. The advantages of this respirator are, that it does not cause the inhalation of rebreathed air (which if not a cause of phthisis is certainly an acclerator.) It does not heat the air. The lint seldom requires renewing or charging. It can be worn day and night without causing any oppression of breathing. It allows of the inhalation of pure air charged to any desired extent with the vapour of carbolic acid. The lint can be dipped at first in carbolic acid solution 1-30; afterwards in a more dilute solution 1-50. Or, carbolic acid and collodion may be mixed in equal portions, and two or three drops of the mixture be placed on the lint. Other antiseptics can of course be also used. (Dr. E. MacDowel Cosgrave, Brit. Med. Jour., Oct. 29.)

CATARRH.—*Carbolic Acid Inhalations.*—In the early stage of catarrh, when the symptoms first develop themselves, such as a disagreeable prickling sensation in the throat, suffusion of the eyes, sneezing, &c., the spray of a 2 per cent. solution of the acid inhaled every half hour is sufficient to cut short and abort the attack. Clothes hung over the head of the bed, saturated with carbolic acid solution of 20 per cent., materially relieve subacute catarrhs of the larnyx and pharynx, and quiet troublesome nocturnal cough. In asthma, following an acute or chronic catarrh of the bronchi, this treatment is most surprising in its results. This treatment is not applicable to acute cases where the parts are very red, the secretion profuse, the cough violent, as in phthisis. (Dr. F. W. Warren, Medical Press and Circular, Jan. 11.)

HOOPING COUGH.—*Carbolic Acid Inhalations.*—Perhaps if there is one disease more than another in which the acid is useful by this method of inhalation, it is hooping-cough. I now in every case use the atomiser, disseminating the spray of a 20 per cent. solution about the room several times through the day, and hanging flannel cloths dipped in the solution over the head of the bed at night. In my hands this treatment invariably cuts short the catarrhal stage of the disease, and abates the paroxysms of the spasmodic stage. It has the great advantage of being easily used, even with the very youngest children. Dr. Thorner recommends inhalations of the acid from the commencement of the convulsive period. It is advisable, he says, to begin with a one per cent. solution, and quickly to increase the strength to 2 per cent. The urine, he says, must be carefully watched, and the inhalations discontinued as soon as the beginning of intoxication is manifested. In cases of young children, or of great irritation of the air-passages, the children ought to be brought three times daily into a small room in which a carbolic acid spray of the prescribed strength is working. I I have never known any bad results to occur in my own cases. Leenan (St. Petersburg Med. Woch.) strongly recommends the use of carbolic acid inhalations in hooping-cough, and in order that it may have the best effect, he advises that it should be administered during sleep, as it is difficult to ensure that a child should inhale sufficiently long, or enough, of the medicament while awake. Woollen material saturated with a 5 per cent. solution should be hung about the bed. Carbolic acid vapour probably acts by exerting an anæsthetic *influence upon the hyperæsthetic mucous membrane of the air-passages.* (Dr. Warren, Med. Press and Cir., Jan. 11.

PHTHISIS.—*Kind of Cases which do best at High Altitudes:* Ordinary chronic tubercular disease, provided the mischief be limited to the upper lobe of one lung, the constitutional powers good, and pyrexia absent, is well suited; the question of consolidation or excavation not being of much importance if the lesion be limited and a large respiratory surface be not involved. To this it may be objected that this is just the class of phthisis which does well anywhere, in hot or in cold climates; and that, in selecting this form, I simply select the most favourable cases. I admit this, but then I expect greater results from high altitude treatment than I have ever found arise from other climatic treatment. My experience leads me to conclude that one, or at most two, winters will cause an entire arrest of the above form of consumptive disease, and that the patient will be able afterwards to resume active life in England. Another class is hemorrhagic phthisis, which gives remarkably good results at Davos. A third is phthisis originating in pneumonia or pleuro-pneumonia, provided only one lung be attacked, and only part of this not undergoing fibrosis. The *cases in which mountain treatment is contraindicated* are: all forms of phthisis, acute or chronic, accompanied by pyrexia; also all cases of either advanced or of extensive tubercular disease, the objection being that such patients have neither the power of circulation to resist the cold, nor the amount of lung-surface necessary to aërate the blood in the rarefied atmosphere; also cases of phthisis in which catarrhal symptoms are the chief feature. These often improve greatly in general health at Davos, but the cough, instead of diminishing, increases; and in one of my patients of this class, catarrh of the larynx, causing obstinate aphonia, came on, this symptom subsiding later on during the hot weather in England. Such patients flourish in moist warmth, and are therefore more fitted for sea-voyages, or for Madeira; and the dictum of Biermer—the south heals catarrh, but the Alpine climate improves the constitution—has amply been borne out by my experience. It is the cold that affects this class of patients unfavourably; and it is possible that, where warmth is combined with altitude, as in the South African health-resorts or in the Andean, such as Quito or Santa Fé di Bogota, they may thrive. What are patients to do when the snow melting necessitates their quitting Davos, which is generally the case at the end of March or the beginning of April? It is the custom to send them to Baden-Baden, or to one of the towns on the north-eastern bank of the Lake of Geneva, there to remain either till the summer commences in England or till the valley of Davos is free from snow. I do not hold with this practice at all, for I think it undesirable to place patients who have been undergoing a strictly bracing climatic treatment, under conditions which are more or less relaxing, such as at Baden-Baden, with its cauldron of hot springs, or on the banks of a lake, where moisture must be abundant in the atmosphere. The descent from an altitude of 5,000 feet should be made gradually, for fear of inducing hæmoptysis, which, though rare at Davos, occurs sometimes in patients after quitting it. The route to be followed is the Landwasser road to Thusis and Coire, and a halt of fourteen days should be made at the former place, which, standing at a height of 2,448 feet at the mouth of the Via Mala, and provided with fair hotels, is a good intermediate station. The next halt should be at Berne (1,765 feet) for a week, and the patient can then return slowly to England, reaching this country in the beginning of May, where, with a well-braced system and with improved powers of taking exercise, he or she will not fear, but rather approve of our cold winds. This plan I have tried for now two seasons, and have found it fairly successful, and would recommend it to other physicians. (Dr. C. T. Williams, British Medical Journal, Dec. 17.

performed it is more likely to lead to renewed effusion or suppuration. Complete evacuation, as advocated by the followers of Trousseau, may bring about a sudden cure, but a survey of many recorded cases proves that the greatest success has followed early, cautious, and partial removal of the fluid, repeated, if necessary, until absorption has set in.

The open treatment.—Under this plan, which is preferable in sero-purulent and purulent effusions, and necessary in their several grave complications, paracentesis with a large trocar and canula is a safe and less formidable proceeding than an incision with a scalpel, which is a more efficacious though more serious operation. In either plan, the strictest antiseptic precautions should be taken in operating. The use of the spray and free drainage into antiseptic dressings, together with suitable irrigations and injections, if these be advisable, are to be recommended. The point of entrance into the pleural sac, if in the mid-axillary region, provided there is fluid there, should not be lower than the sixth space on the right side, and the seventh on the left, which places, as already stated, are about on a level with the common spot of "spontaneous perforation," in the fifth interspace in front. But I advocate the selection, at all events in the adult, of the fifth space, at or as near as possible to that spot, regard being necessarily had to the position of the pericardium on the left side. (Prof. John Marshall, p. 97.)

TRACHEOTOMY.—*Membranous Laryngitis.—Dr. Golding-Bird's Trachea Dilator.*—In membranous laryngitis after tracheotomy has been performed, the case resolves itself, as far as the surgeon is concerned, almost entirely into one of foreign body in the trachea and bronchi, that is if the membrane has already reached so far down; if not, then the dyspnœa will be for the time quite relieved by the operation, but in due course the disease having reached the lower parts of the windpipe, symptoms of a foreign body obstructing respiration again appear. Usually on the opening of the trachea a suitable silver tube is at once inserted, as much with the idea of stopping blood getting into the trachea as of relieving the dyspnœa. For some time after, the child coughs up clots and blood-stained mucus, with perhaps some membrane, whilst the greatest anxiety is felt lest the tube should become blocked, and careful cleansing by feathers, &c., has consequently to be carried out at very frequent intervals; and the patient often does not even then breathe satisfactorily. Such a mode of procedure is *mechanically* wrong, to say the least of it. Everything save air and a minute amount of mucus in the windpipe is a foreign body; and such are the blood and mucus, as well as the membrane, in every croup case. These can scarcely be expelled *viâ* the glottis, for that is already blocked by membrane, while the chances of their finding their way out through a lateral, rigid, and bent tube, the end of which hangs freely in the trachea, like the stem of a funnel in a wide-mouthed bottle, are very remote. If the tracheotomy tube were of the same calibre as the trachea itself, expectoration would be more easy; but it is not only smaller of necessity, but hangs (or should hang) freely in the lumen of the trachea, and with some degree of mobility. That blood and mucus—let alone the more solid membrane—will be readily forced by natural efforts exactly through the mouth of this tube is unlikely, clinical experience shows; and then these foreign matters collect around or below the tube, and are sucked backwards and forwards for long before they hit the right path; while the futile efforts of a piece of membrane to make its exit is announced by a valve-like flapping noise, familiar to all who have had much to do with tracheotomy in croup. Until, therefore, the treachea has been cleansed as far as possible from all foreign bodies, the tracheotomy tube should not be inserted, but other means should be adopted to keep the tracheal wound patent. In eight cases during the last nine months I have acted upon this rule, employing at first the ordinary German nose speculum, as doubtless others have done, instead of a tracheotomy tube, the blades of the instrument being inserted in the tracheal wound, and then screwed open. Although fairly self-retaining, there were many reasons against its being perfect for the purpose, and so the tracheal dilator (see woodcut, p. 188) has been designed. It is inserted, with the blades screwed together, into the trachea, the handle lying against the top of the sternum; on rapidly turning the screw, the blades separate to the required extent, and their shape renders them self-retaining. If desired, tapes can be tied to the blades where they join their handles, and fastened round the neck, but this is not often requisite. The blades retain their parallelism even when fully open, as they do not work from a hinge but travel along the double screw. The same sized dilator does for all cases, whether infants or adults. In any case it may take the place of a tracheotomy tube on an emergency, even when no foreign body has to be extracted. After tracheotomy in every case of membranous laryngitis two distinct mechanical problems are presented to the surgeon :—(1) The establishment of a free passage through which membrane, &c., may be expelled by natural efforts; or, this failing (2) its removal by artificial means. In either case, as a preliminary a sufficiently free opening into the trachea must be made—five-eighths of an inch even in infants, and proportionately longer in older persons. The Tracheal Dilator is made by Millikin and Down, St. Thomas Street, London. (Dr. Golding-Bird, p. 187.)

New Tracheotome.—One difficulty in the operation of tracheotomy is always the introduction of the tube; the operator may pass it in front of the trachea, or miss his opening, and have to make a second, and may lose valuable time, and only succeed after several attempts. To obviate these difficulties, this knife has been devised by Drs. White and Edmunds. It resembles somewhat a pair of dressing forceps; at that end of the instrument which is to be introduced into the trachea, one of the blades has, at its lower half-inch, a cutting edge, and is shaped like an ordinary scalpel: the other, narrow and blunt, lies, when the instrument is closed, in contact with the cutting blade, and well behind its edge. At the handle the blades are widely separated, roughened to give the operator a better hold, and connected by a catch and spring, enabling them to be fixed at any degree of approximation. The method of using the instrument is as follows. The trachea having been exposed, either by the cutting edge of this instrument, or an ordinary scalpel, the instrument is held, with the cutting edge upwards towards the patient's chin, and the trachea opened in the usual manner, the instrument is held in position, and its two handles are pressed together, and the two blades thereby separated, thus widening the tracheal opening, the catch holding the handles in the position to which they have been brought. The patient can now breathe easily. The trachea being held widely open by the "tracheotome," the child should be allowed to expel any membrane it can, and the surgeon has ample opportunity of inspecting the back of the trachea, and of removing any loose membrane seen lying there. An ordinary tracheotomy tube can now be introduced with the greatest ease between the two widely-separated blades in the trachea, and they can be withdrawn as soon as the tube is in position. The advantage claimed for this instrument is that the operator never loses his hold on the first opening he makes in the trachea. It is made by Messrs. Millikin and Down. (*See woodcut*, p. 190.)

TUBERCULOSIS.—Tuberculosis (including all the varieties of consumption in man, the corresponding disease in the ape, and the *Perlsucht* of cattle) is a parasitic disease of the internal organs; the parasite is a bacillus, which may be distinguished from all other bacilli by its remarkable behaviour towards the colouring reagent "vesuvin." It is not the actively moving tubercle-organism of Klebs, for it is stationary; it is not the spherical micrococcus of Schüller nor of Toussaint, for it is rod-shaped; it is not the rod-shaped organism which Aufrecht found (along with two sorts of micrococci), for that was only twice as long as it was broad, whereas the length of the new bacillus exceeds its breadth five times. The tubercle-bacillus of Koch is a slender, rod-shaped, motionless body, in length one-quarter or one-half, or the whole diameter of a red blood-corpuscle, with or without oval "spores" clinging to it, and approximating in morphological features, on the whole, most closely to the "bacillus of leprosy," although the latter is a little more slender, as well as pointed at the ends. How does Dr. Koch arrive at this remarkable conclusion, so pregnant with importance to the well-being of mankind? Dr. Koch does not seek to disguise the fact that he approached tuberculosis purely from the germ-theory side; he does not profess to have taken the entire general pathology of tubercle within the purview of his study. He frankly set to work to look for a minute organism in tuberculous formations of various kinds, and, after a good deal of disappointment, he at length stumbled on the suitable procedure for bringing the existence of the organism to light. The tuberculous substance was either spread out upon a cover-glass, dried, and exposed to heat, or a piece of a tuberculous organ was placed in alcohol and afterwards cut into fine sections. A particular solution of methylene-blue was made, a weak solution of potash being added; the cover-glass coated with tuberculous matter (or a section of the organ) was then placed in the solution for twenty to twenty-four hours, but half an hour sufficed if the solution were warmed in a water bath up to 40° C. The cover-glass, which comes out a deep blue, is then treated with a concentrated watery solution of "vesuvin" for one or two minutes, and is afterwards washed with distilled water. The blue of the methylene has visibly changed to brown; under the microscope all the amorphous detritus and fragments of tissue spread out on the glass are brown, *but the tubercle-bacteria remain blue.* (Ed. Med. Times and Gazette, p. 84.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Dysentery.—*Nitrate of Silver Enemata.*—In the treatment of ulceration of mucous membranes within reach of sight and touch, all practical physicians and surgeons are convinced of the great importance of local applications, whether the seat of the disease be the pharynx, the larynx, the eye, the cervix uteri, or elsewhere. In these situations, whether the ulceration be of constitutional or local origin, we employ local treatment, with or without internal medication, and as the effects of treatment can be observed, no one doubts its efficacy. All practical surgeons are assured of the beneficial influence of applications of nitrate of silver, and other mineral astringents and mild escharotics in treating inflammations of mucous membranes. How different is the treatment of dysentery from that of inflammation of the upper part of the alimentary tract. It may, of course, be remarked that treatment which is obviously beneficial to such parts as our hands can reach, cannot, on account of physical difficulties, be applied to the length of the colon. But this difficulty is not wholly real. With a view of rendering our practice in the treatment of dysentery more successful, and more in accordance with our procedures elsewhere, Dr. Horatio Wood, of Philadelphia, has suggested the use of a large enemata of nitrate of silver, so as to bathe with a solution of this salt the whole mucous membrane of the colon. (Dr. S. Mackenzie, p. 108.)

HEMORRHOIDS.—*Surgical Treatment.*—As a preliminary measure, the functions of the sphincter were suspended by forcible dilatation. Two thumbs were introduced into the rectum, and the circumference steadily kneaded in every direction until all resistance was overcome, and the sphincter rendered absolutely passive. The patulous condition of the rectum thus obtained enabled the whole mass of piles to be protruded through the anus, by introducing two fingers into the vagina and depressing the recto-vaginal wall. The hemorrhoids in size and appearance resembled an average ripe tomato, and were mapped on the surface into four irregular and unequal lobes. The lobes were next divided into four segments by longitudinal sections in the axis of the bowel, and in the furrows marking the intervals between the several lobes. This was accomplished without the loss of any blood. Each portion was then secured in succession by ring-forceps, and dissected with scissors; first, transversely from the anal margin, and then the dissection was continued upwards in the cellular plane to the highest limit of the hemorrhoidal growths, in this case about an inch and a half. Each segment was thus converted into a quadrilateral wedge-shaped mass, the base below consisting of the hemorrhoids, and the apex above of the healthy mucous membrane of the bowel. The mucous membrane at the highest point was next transversely divided, leaving the hemorrhoids simply attached by loose cellular tissue, and the vessels proceeding from above and supplying the mass below. The forceps containing the hemorrhoids were then twisted until a connection was severed and the hemorrhoids removed. The divided surface of mucous membrane was next drawn down, and attached by several fine silk sutures to the denuded border at the verge of the anus. When the patient is in position, I generally commence by compressing a soft sponge about the size of an orange, and passing it six inches or more up the rectum; this precaution prevents the escape of liquid fæces during the operation; and it is hardly necessary to state that the sponge must not be forgotten at the completion of the operation. During the operation, sponges wrung out of spirit and water, one in six, will be found superior to water alone for purposes of clearing blood from the surface of the wound. In operating on men, the hemorrhoids must be hooked down with a finger, or secured by ring-forceps and withdrawn. Attempts to obtain a prolapse of the hemorrhoid by the efforts of the patient, or the use of an enema previously to the administration of an anæsthetic, are undesirable and opposed to sound principles. (Mr. W. Whitehead, p. 204.)

Treatment of Hemorrhoids by "Crushing."—The bowels should be well opened by the aid of some purgative—perhaps the simplest is either Glauber's salts or the compound senna mixture—and, in addition to this, it is usually advisable to give a common enema just prior to the time fixed for the operation, and to steam the parts in the usual manner. The patient having been placed under the influence of ether, should be turned on one side, usually the left, with the buttocks well drawn to the edge of the bed, and with the knee remote from the bed, well drawn up towards the abdomen. The pile to be removed is to be drawn down by means of a pronged fork or vulsellum. The clamp is then to be applied to the base of the pile, and at once tightly and firmly closed by the action of the screw at the end of the handles. The portion of the pile which protrudes inside the lips of the clamp is then to be removed by a pair of scissors curved on the flat or scalpel. After the clamp has been allowed to remain *in situ* for about a minute, the thumbscrew may be gradually slackened; but, at the same time, particular care must be taken to press the jaws of the instrument well up against the buttocks, so that the surrounding tissues may not be unduly stretched, otherwise they will return into the bowel with an "elastic recoil," and will thus have a tendency to tear asunder the "fringed remnant" left by the clamp. The success of the operation being rendered bloodless depends in no small measure on his taking this precaution. The process is, of course, to be repeated according to the number of masses to be got rid of. If several piles are to be removed it is advisable to operate first on the lower, or those situated more externally, and then to proceed upwards; for by adopting this method the "fringed remnants" left by the clamp from the former masses will not be subjected to so much tension when the latter are operated on. If a pile be of an unusually large size, so much so that it is apprehended that a portion may overhang the sides of the jaws, clamp the mass in two portions; but before releasing the grip of the jaws from the first portion, divide only a part of it which is within the grip of the jaws, and then take particular care to have a firm hold with the vulsellum of the portion not yet operated on. Occasionally it may be advisable to separate the skin before clamping the pile. (Mr. R. F. Benham, p. 209.)

Injection of Carbolic Acid into Painful Hemorrhoids.—I have used carbolic acid as an injection into painful and inflamed hemorrhoids, in six cases, with success in every case. As recommended by Dr. Pooley, in the Toledo Med. and Surg. Journal, Nov. 1877, I injected with an ordinary hypodermic syringe, deep into the substance of the pile, three or six drops of crystallised carbolic acid, liquefied by heat. Each hemorrhoid was injected in succession. The pain was slight and soon ceased. The carbolic acid caused a slough which soon separated, and the cure was accomplished in a week or ten days. The cases to which this treatment applies are those of inflamed or irritable hemorrhoids, either external or partly contained within the rectum. This treatment is contra-indicated for internal bleeding piles and for those wholly external that are flabby and comparatively harmless. Prof. Andrews lays down the following rules for the treatment of piles by injection of carbolic acid:—1. Inject only internal piles. 2. Use dilute form of the remedy at first, and stronger ones only when these fail. 3. Treat one pile at a time, and allow from four to ten days between the operations. 4. Inject from one to six drops, having smeared the membranes with cosmoline to guard against dripping; inject very slowly, and keep the pipe in place a few moments to allow the fluid to become fixed in the tissues. 5. Confine the patient to bed the first day, and also subsequently, if any severe symptoms appear. Prohibit any but moderate exercise during the treatment. (Dr. F. W. Warren, Med. Press and Circular, Jan. 11.)

HERNIA.—*Ligature of the Sac.*—The essential part of the operation consists *in ligaturing the neck of the sac close up to the internal ring so as to make the peritoneum flush over the inner opening.* Unless this is done effectually the operation will fail. I make a linear incision over the neck of the sac, and in a line with the direction of the hernial protrusion, cut through the coverings of the hernia until the sac is reached, reduce the hernia, and open the sac sufficiently to allow a purified finger to be inserted into the neck. The finger is used to certify that the sac is clear of intestine and omentum, and to indicate the position of the ligature. The neck of the sac only is now freed from the surrounding tissue by a director or probe, and a double catgut ligature passed round it by means of an aneurism needle. If the hernia is an inguinal one in a male, the cord is separated from the sac and excluded from the contents of the ligature. The ligature is now tightened as the finger is withdrawn, and knotted in the usual way. I then cut through the neck of the sac below the ligature, and close up the wound, but not so completely as to prevent free exit for any exudation. The operation is performed with all the precautions of the spray-gauze method. Such is what I consider the typical operation for hernia, by ligaturing the sac. The excised sac has vital connections with the surrounding tissues, and does not slough, and the cutting through of the sac produces a firmer union than would occur were the sac only ligatured. But the sac may be completely extirpated, or its neck may be only ligatured and otherwise not interfered with; or the sac may be raised from its bed and allowed to slough, and each of these modications may have their suitable cases. (Dr. W. Alexander, p. 196.)

Spanton's Operation for Radical Cure of Hernia. — Surgeons may differ as to the propriety of attempting a radical cure, or as to the particular operation they may select, but there can be no dispute about one point, that all operations in use previous to the publication of Mr. Wood's most admirable monograph in 1863, were either extremely dangerous—or even barbarous—or that they failed to effect a complete or permanent cure in the vast majority of cases. Since Mr. Wood's operation came into notice, it seems to me that but one other procedure can claim to be discussed with it, I mean that of Mr. Spanton. The operation devised by Mr. Spanton is performed with an instrument something like a corkscrew, but with a flat point, so as to pass through the fibrous structures without doing much violence to them; it is broader near the point than at the handle, so that as it is screwed onward into the boundaries of the canal, it approximates them to each other. The same preparation of the patient is made as for Wood's operation, by shaving the skin, incising it, and separating it from the subcutaneous tissue to such an extent as may be necessary to permit the invagination of the latter. The rupture being reduced, the invagination is effected by the index finger of the left hand, when the screw, held in the right hand, is made to pierce the skin of the groin, and the aponeurosis of the external oblique muscle at a point above the outer border of the conjoined tendon, through which it is then passed, by giving it a half-turn. It is then twisted towards its axis, being directed downwards and inwards in such a way that as its point crosses and recrosses the canal it passes first outwards through the invaginated sac and the external pillar, then inwards so as to catch the internal pillar of the ring, and finally outwards again so as to pass through the scrotal wound in the skin. The part is then dressed and bandaged antiseptically. The instrument is usually removed in about a week, and the wound allowed to heal. The advantages claimed for this operation by its author are: that instead of only catching the boundaries of the canal at two or three points, as in Wood's operation, it seizes them at a greater number, and owing to this, to the numerous points at which the sac is transfixed, and to the compelling action of the screw in drawing the parts together, it effects a more complete closure of the canal. The plug, too, is described as more cylindrical in form, less conical, and therefore less likely to slip downwards from its position. As in Wood's operation, the finger which performs the invagination is used to direct the point of the screw through the proper tissues, and to protect the cord and deep epigastric artery from injury. (Mr. W. T. Stoker, Med. Press and Circular, Feb. 27.)

After the usual preparation in such cases, ether being given, standing on the left of the patient, an incision was made in the skin of the scrotum large enough to admit one's finger, downwards and outwards, and the skin separated for about two square inches in extent. The hernia having been previously reduced, the left forefinger was passed into the scrotal wound, pushing before it sac and scrotal fascia high up into the inguinal canal, so that the finger could easily make out the condition of the abdominal rings and surrounding structures. The invaginated tissues being so held by the finger, the point of the screw was, with the right hand, thrust through the skin of the groin so as to pass through the conjoined tendon at the internal ring in such a way that the point came against the tip of the left finger. The screw was next made to transfix the sac and fascia held in the inguinal canal, and so to pass across to the external pillar subcutaneously. A turn was then given to the screw, so as to push it through the internal pillar of the external ring and again across to Poupart's ligament, the point emerging through the scrotal fascia at the wound. In this manner the sides of the hernial canal are approximated throughout, and the plug of invaginated tissue is firmly held in position while consolidation is taking place. At the same time the general cavity of the peritoneum is securely shut off. The left finger protects the spermatic cord throughout, and is withdrawn as the screw traverses downwards, the point of which should be felt by the finger from first to last. The handle of the screw then lay flat on the abdomen, and the point was protected by a ball of India-rubber, and the scrotal wound closed by a suture of catgut. There was no hemorrhage worth mentioning. Carbolic spray and Lister's dressings were used. The screw was removed on the 9th day, and the dressings relinquished on the 15th day. He was allowed to get up on the 28th day, the parts being firmly consolidated, and no impulse was felt on coughing. Two days afterwards he returned home. (Mr. W. D. Spanton, p. 201.)

NUTRIENT SUPPOSITORIES.—In the British Med. Journal of Feb. 19, 1881, is a short description of a plan of rectal feeding which appeared to me to have many advantages over the usual one, of enemata. It is unnecessary to point out the several objections to the latter. My suggestion, based on many experiments, resolved itself into this. Artificially digested meat is mixed with a little wax and starch, and made into a suppository. (The practical difficulties of the process were successfully overcome by Mr. Frank Slinger, F.C.S., of the firm of Slinger and Son, of York.) These suppositories are of such a size that the digested and extracted product of twenty ounces of meat from which the insoluble matter is removed, is contained in about five suppositories. The convenience of this method is very great. It is easy for most patients to introduce them themselves; and their use is attended with no discomfort whatever in the majority of cases. After an hour or two the waxy basis is frequently returned, the peptone and extractive being absorbed. In some few cases, owing to irritability of the rectum, the whole suppository has returned; but this can be obviated by the addition of a little opium to each suppository. (Mr. H. E. Spencer, Practitioner, Feb.)

PERITONEAL SURGERY.—1. All hemorrhages must be promptly controlled by pressure, ligature, or hæmostatic forceps. This principle is common to all operations. 2. The peritoneal cavity must be thoroughly cleaned after operation, and before the abdominal incision is closed. This is the great lesson taught by Thomas Keith, and followed by all successful operators. 3. The abdominal incision, usually in the middle line, must be properly closed. 4. Drainage or no drainage. Chassaignac was the first to demonstrate the importance of drainage in general surgery, and no one now pretends to dispute its value. He was the first to point out the source and dangers of septicæmia and pyæmia, and at the same time to designate a preventive in his *tubes à drainage.* The drainage-tube is now wholly excluded by Spencer Wells, Thornton, and many others, on the theory that Listerism renders the peritoneal effusion aseptic, and therefore that its absorption will not be attended with danger. The only valid objection that can be urged against the drainage-tube in abdominal surgery is, not in its immediate danger, but in its ultimate tendency to develop ventral hernia. And this is a serious objection, which we, who advocate its use, must learn to obviate. This is a problem to be worked out, and I am sure it can and will be done. But till then it is better, in doubtful cases, to risk the production of ventral hernia, with all its inconveniences, than to risk the life of the patient. (Dr. Marion Sims, p. 191.)

TAPPING.—*A New Antiseptic Trocar.*—A new trocar which I desire to introduce to the notice of the profession is a very simple and handy instrument. The special feature in its construction consists in this, that it is perfectly air-tight, and can be opened and shut within an india-rubber case (*see woodcut*, p. 186). It is composed of two metal tubes; the outer carries a pen-shaped lance, and the inner is round at the point with a lateral opening. By a bayonet-joint the trocar is opened and shut, at the same time the point of the lance is protected from injuring any internal part. The orifice and joints are introduced into the enlarged end of the India-rubber tube, which is slipped over a metal block, and then secured by a ring, and this serves for a handle to the instrument. It is adapted for all kinds of tapping; it can be used also for exploring or injecting purposes, or it can be very readily attached to the exhausting apparatus of the aspirator. (Dr. J. Ward Cousins, p. 185.)

AFFECTIONS OF THE URINARY SYSTEM.

BLADDER.—*Digital Exploration.*—It is possible in not a few cases to explore through a small perineal incision the whole, or nearly the whole, of the internal surface of the bladder with the index-finger. A necessary condition, of course, is that the bladder should be empty; and in that condition it is—as is sometimes perhaps not sufficiently recognised—no longer a cavity, as represented in diagrams, but a mere cul-de-sac at the end of the urethra. Taking it for granted that the incisions to be made, which involve only the urethra with the tissues between it and the surface, and not at all the bladder and prostate, enable the operator to place the last joint of his left index-finger within the neck and thus to draw it somewhat towards him, the exploration may be accomplished in the following manner. Maintaining his finger at the spot described, the operator should stand up at the foot of the table, a little to the left of the patient (who is in the lithotomy position), so as to make firm pressure with the right hand above the pubes, the resistance of the abdominal muscles being overcome by the influence of ether. He may now easily feel, unless the patient be very stout, the opposite side of the bladder coming into contact with the tip of his finger; and by concerted movements of supra-pubic pressure by the right hand, with slight movements of the left index in the bladder, almost every portion of the internal coat of the latter may be brought under examination. If the patient be thin, the proceeding is easy; it becomes less so in proportion to the depth of perineum and thickness of the supra-pubic coverings, both of which are increased by fat. In the latter case aid may be rendered by an assistant, who makes firm pressure with both his hands, but the operator should also employ, if he can, the concerted movements described. (Sir Henry Thompson, p. 222.)

CATHETERS.—*New Form of Guide Catheter.*—Ever since the introduction into urethral surgery of "guide bougies," various plans have been devised for removing the conductor whilst leaving the catheter in the bladder; but the methods employed seem not to have met with complete success. By a simple mechanical contrivance this difficulty has been overcome in the instrument mentioned below, whereby the risk of the conductor being broken off in the bladder is reduced to a minimum. The instrument consists of an ordinary metal catheter, the point of which is perforated by a small hole sufficiently large to permit of the passage of the "ferret." The stilet consists of a gum elastic bougie, similar to that commonly in use, and to which the attention of the profession has recently been drawn by Mr. Teevan. To the end of the bougie is riveted a metal point, accurately moulded to the interior of the catheter, and in which is sunk a female screw. The ferret is of the ordinary form, with a well-made screw-mount. In using the instrument the ferret is first passed into the bladder. The catheter, with the bougie *in situ* is then screwed on, and, owing to the accurate moulding of the bougie-tip to the interior of the catheter, this is an easy matter. Once in the bladder the bougie, and, with it the conductor, is withdrawn. The catheter can then be tied in or withdrawn, according to the requirements of the case. In cases of retention of urine, and in all cases of difficult stricture in which a metal catheter is to be retained, the instrument will be found very serviceable. The advantages claimed are greater safety to the patient, a saving of expense, in that it obviates to a considerable extent the use of the flexible catheter a boule, a matter of considerable importance both in hospital and private practice, whilst at the same time the catheter, if required, can be used without the ferret. All the advantages of the solid bougie over the stilet are retained; also the bougie may be utilised as a flexible probe. Mr. Wood, of King-street, Manchester, from whom the instrument can be obtained, has taken considerable pains in its construction; and by adopting a uniform mount to the ferret, damaged ones can be more easily replaced. The smallest size at present made is No. 2 English gauge. (Mr. E. H. Howlett, Lancet, Jan. 14.)

The Davidson Open-Mouth Catheter.—We have received some specimens of the special catheters made by the Davidson Rubber Company, Boston, U.S. The mouth or eye of the catheter is central at the end, and its edge is beautifully smooth. The instruments are made of very flexible red rubber, which has an exquisitely polished surface; this form of rubber is stated to be very durable. The catheters are certainly the best of the kind we have ever seen, their extreme suppleness and smoothness and the terminal opening being important advantages. (Lancet, Jan. 14.)

CYSTITIS.—*Boracic Acid.*—Rosenthal speaks highly of the treatment of cystitis by boracic acid. The following formula is the one to which he gives the preference: ℞. Boracic acid, 2 grammes; water, 100 grammes; syrup. aurant. cort. 10 grammes. This amount is to be taken daily. The mixture tastes like weak lemonade, and may be continued for long periods without producing the unpleasant secondary effects of salicylate of soda, chlorate of potash, and the resins. In the severer cases of cystitis with profuse secretion of mucus or pus—cystitis ammoniacalis—Rosenthal washes out the bladder twice a day with a five per cent. solution of the same agent. (Practitioner, April.)

DIABETES.—*Codeia.*—At the recent meeting of the British Medical Association at Ryde, Dr. S. Shingleton Smith, of Clifton, stated that opium had been empirically used in the treatment of diabetes from the time of Aetius. In recent times the action of the drug has been investigated by Pavy and others, who have shown that the practice is the result of well-established experience; and observers have endeavoured to ascertain to which of the alkaloids of opium the beneficial result is due. Codeia was first recommended by Pavy on the ground that it could be given in large doses without producing drowsiness. Of late it has been much used as recommended by Dr. Saundby in the cough of phthisis, where it gives great relief and has an appreciable soporific effect. But it is in diabetes that codeia has been of greatest service. As regards the dose, small doses are recommended by some authors, but it may be given in doses of a quarter to half a grain, three times a day at first, the medicine being increased gradually until sugar disappears from the urine, or increasing drowsiness demands its discontinuance. Dr. Pavy has given a series of cases in the Guy's Hospital reports showing the beneficial effects of opium, morphia, and codeia in removing sugar from the urine, the advantage of codeia being that it does not produce the same narcotic effect as opium and morphia. Opium is given in doses up to nine grains, morphia up to three grains, and codeia up to ten grains, three times a day. Dr. Cavafy has given fifteen grains thrice daily with good result. Dr. Smith considers that alkalies, and all other treatment, even dieting, are inferior to codeia as remedies for diabetes, and that in this disease it may be considered almost a specific and should be the first remedy tried, and should be given in fairly large doses until some physiological effect is produced. Codeia has been said to produce convulsions, but the literature of the subject does not support this, and Dr. Smith has never seen any such effect. The author related three cases which all exhibited marked improvement while taking codeia, which improvement ceased when the codeia was withheld and was renewed on its repetition. Morphia had a good effect in two of the cases, but the improvement was much less marked than with codeia. (Dr. S. Smith, Practitioner, Jan.)

FIDDLE-STRING AS A BOUGIE.—Dr. F. E. Daniel, of Jackson, Miss., failing in a case of very tight stricture to get in the smallest ordinary bougie, used in the emergency a small *fiddle-string*. This passed in readily. Being withdrawn in a few minutes, it was found to have swollen to nearly twice its previous size. A larger one was then passed and allowed to remain fifteen minutes; this being then withdrawn, the urethra was sufficiently dilated to get in a No. 4, then a No. 6 bougie, and finally a flexible Nélaton's catheter, threaded on a fiddle-string. A second case was equally satisfactory. Dr. Daniel claims for the fiddle-string (catgut) cheapness, simplicity, availability, harmlessness, strength, and rapid expansion. (Dublin Journal, March.)

Hydrocele.—In infants hydroceles are common, and get well spontaneously or after the application of muriate of ammonia and spirit lotion, weak tincture of iodine or collodion. In obstinate cases a few silk threads passed through the hydrocele for a few hours will cure. Tapping is of use to reduce the size of large hydroceles prior to their injection with iodine solution, and for the purposes of diagnosis, and is sufficient when the patient is old. Sir A. Cooper says of the methods of cure by incision and the introduction of lint or flour, or by excision of the sac, they are too cruel and dangerous for so trifling an inconvenience. Caustics are uncertain, and that they are dangerous a specimen in the Hunterian Museum bears witness. The treatment by tents was severe, and caused much constitutional disturbance. Munro, Sharp, and others employed injections, but they used them too strong. Sir James Earle did good service by recalling the attention of the profession to the cure of hydrocele by injection in 1791. Port wine and water were generally used, also spirit, but the solutions varied so much in strength that a solution of sulphate of zinc, one drachm to a pint of water, was substituted, and with this the tunica vaginalis was distended several times, and the fluid allowed to escape. Modern surgeons are much indebted to Sir Ranald Martin for introducing to their notice tincture of iodine, one part to three of water, as an injection. Mr. Curling recommends the following solution: iodine two scruples, iodide of potassium half a drachm, rectified spirits of wine one ounce; inject one or two drachms, which are to be left in the sac; to be diluted one-half for young persons. Sir William Fergusson used equal parts of simple tincture of iodine and water, which he allowed to remain, and found a drachm or two of the solution sufficient for the cure of the largest hydrocele. Mr. Syme used to inject and allow to remain a drachm or two of the Edinburgh tincture of iodine. The cannula is to be introduced and pushed up to its shoulder, and the solution thrown in, taking care, by pinching the tissues against the cannula, to keep it in place and prevent extravasation into the cellular tissue. I have strapped some cases after tapping without preventing the reaccumulation of the fluid. If syphilitic orchitis be present, specific medicines will remove both the orchitis and the hydrocele. It has been proposed to tap with a large trocar and cannula, and through the latter introduce strong carbolic acid or iodoform. Setons, instead of being used as the ordinary method of curing hydrocele, as recommended by Mr. Pott, are reserved for cases in which injections have failed. A double thread is carried across the sac and withdrawn after a few days, when it has set up sufficient inflammation. I have tried silver wire as a drain, but found that it caused a good deal of inflammation, and that its tract was slow to heal. Under these circumstances a good plan is to fix the tunica vaginalis and skin by passing a couple of threads transversely through both skin and tunica vaginalis two inches from one another, divide the parts between the two threads, wash the tunica vaginalis out with carbolic acid solution (one in twenty), and stitch its cut margins to the skin with carbolised catgut. Dress with antiseptic precautions, and use iodoform wool. (Mr. H. Royes Bell, Lancet, Jan. 14.)

Of late I have practised with complete success the following means for effecting the cure of hydrocele. Having tapped the cyst with a short but rather large trocar and canula, when the flow of liquid ceases, I charge a long but slender silver spoon or spatula with a small quantity of finely powdered iodoform, and slip it through the comparatively wide canula into the cavity of the tunica vaginalis, or cyst, as the case may be. A few movements of the spatula will serve to bring the iodoform into contact with different portions of the cyst wall, and, in the event of the tumour having been of large size, a second, or even third, spatulaful of iodoform may be introduced, care being taken to apply some of the powder to the upper and lower extremities of the cyst. After withdrawal of the spatula, the canula is to be removed, when the puncture may be closed by holding the skin pinched between the finger and thumb for a few minutes. The patient experiences but little pain either during or subsequent to the operation, and, although it is not desirable that the scrotum should be subjected to rough usage, yet it is by no means necessary to keep the patient confined in his room. From my experience of the iodoform treatment, I fully anticipate that all who may give it trial will find this method to contrast favourably with the sometimes uncertain, often painful, and always disagreeable injection of an iodine solution; whilst, as for the German plan of laying open the cyst-cavity, and drawing it under antiseptic dressings, I consider the measure an unnecessarily severe one, quite uncalled for in the vast majority of cases. (Mr. P. J. Hayes, Brit. Med. Jour., Dec. 10.)

LITHOTRITY.—In 1878 an important change in this operation was proposed, as is well known, by Prof. Bigelow. He advised that the stone, however large, and without respect to the presence of cystitis or other considerations, should be invariably removed at one sitting, by means of more powerful lithotrites, larger evacuating catheters, and a stronger India-rubber bottle than had before been used. The condition necessary and preliminary to this proposal had been the adoption of a doctrine taught by Otis, of New York, that the ordinary male urethra might be treated as capable of fairly admitting instruments of 16 or 18 gauge, English, or Nos. 20, 30, French; instead of Nos. 12, 14, English, 22, 25, French. My own experience of it, which is therefore considerable, amounts now to 101 cases in the elderly adult, which have occurred in 101 different individuals, and I do not hesitate to regard it as a valuable improvement on any method previously employed. In order to guard against danger, our first care should be to avoid in every case where it is possible to do so, the use of instruments, whether for crushing or evacuating, which over-distend and irritate the urethra and neck of the bladder. At least our rule must be not to employ larger instruments than the size of the stone demands. I cannot too earnestly warn the inexperienced lithotritist against the needless risk he incurs when, in presence of a small or moderate-sized stone (and the majority met with belong to one of these two categories), he uses the heavy lithotrite and the large evacuating tubes which have of late been introduced into this country. In order to remove two or three hundred grains of calculous matter from the bladder—and many calculi weigh less than a hundred grains, while all ought to be found before they attain that weight—it is wholly unnecessary, I will even say it is unwarrantable, to introduce lithotrites and evacuators with the diameter of No. 18 or 20 English scale into the bladder. I am certain that the mere splitting, for it is not dilatation, of the urethra and neck of the bladder, which sometimes follows the introduction of such instruments, has sufficed to produce symptoms often distressingly painful, sometimes obstinate in duration, occasionally fatal. Surely it is more prudent to bestow two or three minutes more on the work, to ensure more complete crushing, so that the débris may be removed by a tube of 15 or 16 English diameter, than to crush coarsely and use an evacuator which infallibly inflicts serious mischief in a certain proportion of cases. I briefly sum up thus:—Lithotrity completed at a single sitting is in experienced hands an operation unequalled in its safety for the patient. It appears also to produce less subsequent persisting irritation of the bladder than the operation by several sittings. No new form of instrument is required by this operation. The value of the proceeding lies altogether in the removal of all the foreign matter from the bladder at once, so that nothing remains to excite inflammation in an organ already irritated by the process. And the less irritating the operation has been, the more certain and more speedy will be the recovery. It should be employed by beginners only for calculi of moderate size when hard. If calculi are large as well as hard, a young surgeon will probably proceed more safely by lithotomy. In friable phosphatic calculi, size offers a much less serious difficulty. Lithotrity at a single sitting for a hard calculus, upwards of an ounce in weight, and *à fortiori*, when double that weight, certainly demands an experienced operator. (Sir Henry Thompson, p. 213.)

PROSTATIC OBSTRUCTION.—*Early Treatment.*—The teaching of the present day is to the effect that mechanical treatment is not to be employed until either retention occurs or the bladder becomes inflamed, when such means may be resorted to. It was asked why the same treatment should not be applied as in the case of urethral stricture. With the view of obtaining similar conditions to those occurring naturally in large prostates, where there is no interference with micturition, a mode of treatment with specially adapted bougies was described. The instruments are gum-elastic, two to four inches longer in the stem than usual, with an expanded portion an inch from the tip, which is made to enter the bladder. In this way the prostatic urethra is subjected to pressure on the insertion and withdrawal of the instrument. As a rule, if dilatation is not too rapidly proceeded with, no irritation is aroused. On the contrary, greater toleration of urine follows, owing to the ease and completeness with which the bladder is then emptied. (Mr. Reginald Harrison, p. 229.)

RENAL TUBE-CASTS.—The following is the best mode of finding and preserving them. If the casts be very numerous, they may be found at once by placing a drop of the urine under a power of 200 or 250 diameters; but, as a rule, it is desirable to put about four ounces of the urine into a conical glass, and allow it to stand for from twelve to twenty-four hours. At the end of that time, a sediment has usually formed; it may be only a slight or scanty cloud, or a more or less copious and dense deposit. If no sediment be visible, few or no tube-casts will be found even after the most careful and prolonged examination. A few drops of the sediment, taken up with a pipette, are then placed in a shallow cell made by cementing a thin ring of glass or metal on a glass slide; or a cell may be made of a ring of cement, such as Hollis's glue or Brunswick black. The cellful of liquid being covered by thin glass, and the excess of liquid removed by a cloth or blotting-paper, is now ready for examination. The examination is much facilitated by the use of the shallow cell. When the sediment is scanty, it is a waste of time to be searching for tube-casts in a fraction of a drop of the urine which alone can be retained between two flat pieces of glass. The basis of all tube-casts is an albumino-fibrous material derived from the blood, which, when it exists alone, forms a transparent structureless so-called "hyaline cast." Hyaline casts may be "small" or "large;" the latter being about twice the diameter of the former, and having a more sharply defined outline. What is the explanation of this difference of diameter? Not, as is sometimes carelessly asserted, the difference in the size of the tubes in which they have been moulded. The vast majority of the casts, both large and small, have been formed within the convoluted secreting tubes of the cortex, which are remarkable for their uniform size. The different diameter of the casts is explained by the fact that the small casts have been moulded within the lumen—the clear central canal—of convoluted tubes which retain their lining of glandular epithelium; while, on the other hand, the "large hyaline cast" has been formed within the basement-membrane of a tube the epithelial lining of which has been destroyed and displaced by the hyaline material of the cast. The large casts, therefore, point to a greater structural change within the tubes than is indicated by the small casts. (Dr. George Johnson, p. 110.)

STRICTURE OF THE URETHRA.—*Treatment by Stretching.*—Stricture-stretching, as I endeavour to carry it out, is very closely in imitation of the process to which gloves are submitted by the salesman after the purchase has been made. Into each stall of the glove the glove-stretcher is introduced, in order that the fingers may the more readily pass in. If the manipulation be dexterously done, neither are the seams split, nor is the kid torn; if clumsily, a rent and a false passage for the finger is the result. So much for the simile, now for the reality. The stretcher I employ is that much abused but valuable instrument, Holt's dilator; and the stretching is effected quite slowly, by the introduction of successive sizes of dilating rods, until the dimensions required are obtained. All this is to be done at one time, with or without an anæsthetic, according to circumstances. I almost invariably have the patient placed under ether, as it is important that the process should be slow enough to permit of the tissues stretching without tearing. I do not think we fully recognise the extent to which the urethra is capable of being distended. Dr. Otis, of New York, has recently demonstrated the truth of Sir Everard Home's observation: "That the urethra is everywhere much larger than had been supposed, exceeding the size of the largest bougie in use in a very great degree." And what is true of the healthy urethra will be found to apply, though in a less degree, to the strictured one. The adaptations in Holt's dilator, specially fitting it for this purpose, may be briefly stated. In the first place, there is attached to it a pilot bougie, which, in cases of tight or tortuous strictures, will be found invaluable. Though the pilot bougie may not be necessary for all cases where stretching is to be employed, yet, as this treatment is intended for some of the worst forms of the affection, it is as well to be prepared for every contingency. If the pilot bougie be not required, a round-top screw takes its place. Then I have increased the number of dilating rods to eight, so that the process of stretching may be gradual; and, in order that there may be no jerking as the several rods are introduced, a spiral spring controls the separation of the two portions of the instrument, instead of a screw. The size of the dilator, before a rod is introduced, is No. 3, English gauge; the largest dilating rod brings it up to rather over No. 12—so that, between these two extremes, we have seven gradations in size. If the stricture is so tight as not to be capable of receiving the dilator, dilatation by means of filiform bougies must first be employed until the required size is obtained. Other instruments, divulsors and screw-dilators, as they are termed, have been invented for a similar purpose; but all I have seen are open to two objections: 1. They either tear or pinch with their sides the mucous lining of the urethra; 2. They are too large for use, except for strictures of the largest calibre. One of the best divulsors I know of is that adapted by Dr. Samuel Gross of Philadelphia. It is provided with an index at the handle, which shows precisely the extent to which divulsion has been carried. A word or two in reference to the use of the instrument I employ for stretching (*see woodcut*, p. 232). The patient being placed under ether, in the recumbent position, the dilator, with or without the pilot bougie, is passed fairly into the bladder, and then the rods are slowly and gradually introduced until a full size is obtained. The process generally occupies from ten to thirty minutes, according to the resistance, care being taken not to tear, and so cause hemorrhage. I then withdraw the dilator, and pass a catheter, for the purpose of removing any urine that may be there. I usually give a dose of Fleming's tincture of aconite immediately after the stretching has been practised, and require the patient to remain in the house for twenty-four hours. A full-sized bougie is then passed every third or fourth day, until the patient has learnt how to conduct this operation himself, when he must be enjoined to continue this practice. It will be seen that this proceeding differs essentially from what is so well-known as Holt's operation. In the one, the object to be attained is stretching; whilst in the latter, it is rupture. It is the only plan of treatment by which the strictured urethra is at once brought up to a full capacity, without break of tissue. (Mr. Reginald Harrison, p. 232.)

Varicocele.—Mr. Erichsen passes a silver wire subcutaneously round the veins, which are constricted and finally obliterated by tightly twisting the loop. An operation which requires to be repeated as it were daily is faulty as a surgical proceeding, and one dreaded by the patient. Mr. J. Wood passes an iron wire around the veins in the usual way, the ends are then passed through a large eye at right angles to the end of an iron rod about the size of a stout knitting needle, which has at the other end an elongated horseshoe-shaped spring of steel to which the wire is attached. The spring acts continuously on the wire, which would, if desired, cut through the veins. To avoid the ulceration caused by the direct pressure of this instrument on the skin, I have devised a small traction instrument which is used in the following way. Pass an unthreaded needle with an eye at its point between the veins and the vas deferens; knot a loop in a piece of stout silk; pass one end of it through the eye of the nævus needle, which is then to be withdrawn and passed, without unthreading it, subcutaneously in front of the veins; now unthread the needle and pass the end of the disengaged thread around the free thread. Do not withdraw the needle, but pass the free end of the thread through its eye; it is now to be withdrawn, the veins will be tied subcutaneously, the ends of the thread are to be passed through the eyes of a small instrument which has four arms that unite at what is called a circular joint (*see woodcut*, p. 237). Traction is made and regulated by twisting a number of turns of an india-rubber band round the distal end of the tractor. The tractor can be readjusted at any time if it gets loose, without pain; it pulls away from the veins and skin, and the small holes made by the passage of the threads soon heal. There is no rule as to the time ligatures are to be left round the veins, but the usual time is eight or ten days. I have met with good results after the operation with the elastic ligature, which may be passed round the veins subcutaneously by means of a large sewing needle; the elastic cord is to be stretched to the utmost, and then clamped by means of a leaden circle, which is pushed well home. The galvanic cautery is used by Mr. Pearce Gould, of the Westminster

Hospital, for the cure of varicocele. The veins are isolated from the cord, a narrow-bladed knife passed through on the flat between the veins and the vas deferens, a needle threaded with a platinum wire is then passed along the tract of the knife behind the veins, and subcutaneously in front of them. The wires are then connected with a galvanic écraseur, become red hot, and in a few minutes cut through the veins. The operation is bloodless and radical in its nature, the skin is prevented from being charred by dropping a little water at the point of exit of the wires. A small nodule is to be felt later on where the veins are divided. Varicocele is attended at times with a good deal of neuralgic pain, especially after much standing or muscular exertion, which distracts the patient's attention, and causes more mental worry than its presence warrants. In adults the testicle may feel softer than normal, but in young cases Mr. Gould states that the testicle is small and undeveloped, not wasted. The operation for the cure of varicocele is not formidable, is attended by little pain, and is free from danger. (Mr. H. Royes Bell, p. 235.)

Mr. Harrison's operation consists in exposing the cord by a vertical incision about an inch in length and separating the veins which are most prominently varicose. These are each tied in two places with a catgut ligature. Usually three or four main veins require to be so dealt with. In addition, it will be observed that there are a number of small veins in plexuses or bundles, especially in close proximity with the epididymis, which cannot be obliterated in this way; these he destroys with a few light touches of the thermo-cautery, taking care, while this is being done, to protect the vas deferens by holding it from behind, between the thumb and index finger of the left hand. The operation is conducted antiseptically. No sutures are introduced, and the wound is left to heal by granulation. He has now been performing this operation for some years, and is not aware of an instance in which there has been a return of the varicose condition of the veins; the cicatrix which results is of a contractile nature, such as usually follows wounds inflicted by burning, and gives a feeling of firmness to parts which previously were complained of as being unnaturally lax. It is a matter of great importance, in view of preventing a return of the disease, to destroy the small varicose veins, which can only be seen and not felt. If this is not done, obliteration only means driving the disease into new channels, already preparing to dilate. (Mr. Reginald Harrison, p. 238.)

FRACTURES, DISLOCATIONS, AND DISEASES OF BONES, JOINTS, ETC.

Antiseptic Treatment of Wounds.—The serum of blood is not at all so favourable a soil for the growth of micro-organisms as I had previously imagined. If we take a glass of uncontaminated milk or urine, so arranged that if left untouched it will remain for any length of time free from organisms, and add to it a drop of ordinary water, we are sure to find in a few days evidence of bacteric development in the liquid. Indeed, in the case of milk, which appears to afford pabulum for almost all varieties of micro-organisms, I have shown that if a dozen glasses of the liquid in a state of purity, in vessels suitably arranged to prevent contamination from without, receive each one-hundredth of a minim of tap water, most of the glasses will develop bacteria, though of different species in the different vessels, showing how numerous and how various are the micro-organisms really present in water. Even the comparatively crude liquid which we call "Pasteur's solution," a mere solution of cane-sugar, tartrate of ammonia, and earthy salts, in which many kinds of bacteria refuse to grow at all, will be pretty sure to produce such organisms in a few days if a drop of tap water is added to it. Now from these analogies, and knowing as we do to our cost that blood serum is but too liable to putrefactive fermentation, I had assumed that ordinary water contained putrefactive bacteria in a form that would develop in serum. But when, in the course of an experiment I drew blood, with antiseptic precautions, from the jugular vein of an ox into a series of purified bottles, about half an ounce into each, and, having allowed the blood to coagulate and the clot to shrink, introduced various quantities of tap water to mingle with the expressed serum in the several vessels, I found to my surprise that not only an entire minim, but two, four, and even eight minims, failed to induce putrefaction, although the bottles were kept in a warm box at the temperature of the body. I have since confirmed this experiment in the ox, and have also extended it to the blood of other animals—the donkey and the dog—with similar results. I even found that putrid blood in full activity, if largely diluted with water purified by boiling, and introduced in small quantity in proportion to the serum, failed to occasion putrefaction or the development of any organisms that I could discover by ordinary microscopic examination. Yet the same quantities of the same dilutions quickly gave rise to putrefaction in the blood of the same animal altered by mixing it with an equal part of purified water, showing that they really possessed septic energy, though unable to exert it upon normal serum. Not that the blood of these animals was in its natural state incapable of putrefaction, for inoculation with a very small quantity of undiluted putrid blood soon rendered it highly offensive. But the results of these experiments seemed to point to the remarkable conclusion that, after having been widely diffused by means of water, bacteria are incapable of developing in undiluted healthy serum. In this respect serum is totally different in its behaviour from milk, in which, as I have shown elsewhere, a single *Bacterium lactis*, detached from others by a similar process of diffusion by means of water, is as sure to produce its kind as are a million taken directly from souring milk. How it is that the diffusion of bacteria renders them incapable of developing in serum I do not profess to understand. It may perhaps be that when bacteria are introduced directly from putrid blood, the products of the putrid fermentation adhering to them may induce chemically an alteration in the normal quality of the serum which, when thus impaired, may prove amenable to the nutritive energies of the micro-organisms, while conversely copious ablution with water may remove from the bacteria the associated substances which thus act as their pioneers. This view might be otherwise expressed by saying that the bacteria *per se* are unable to grow in normal serum, and can only develop in that liquid when it has been vitiated, whether by the addition of water or by the action of small quantities of the acrid products of putrefaction. Applying this knowledge to the discussion of ovariotomy performed without antiseptic precautions, the question naturally suggests itself whether in many cases any septic organisms have really been introduced into the peritoneal cavity, either from air or from water, in a condition capable of developing in the effused serum. And thus we have suggested to us a further explanation of the success of such operations. But the facts which have been elicited by the experiments referred to have a far wider range of application than to the special case of ovariotomy. They seem to indicate that the putrefaction so apt to occur in wounds not treated antiseptically is due rather to septic matter in a concentrated form than to the diffused condition in which it exists either in water or in air. They suggest the highly important question, *Is the spray really necessary?* In other words, Is there sufficient chance of the air of an operating theatre or private room containing septic matter which can prove effective in blood serum to make it needful to regard the question of contamination from the atmosphere at all? If the answer must be given in the affirmative, and the choice must lie between the spray and antiseptic irrigation during the operation at intervals varying according to the discretion of the surgeon, with syringing of the cavity of the wound after stitching, and syringing also at every dressing, then I should give my voice decidedly in favour of the spray, as being more sure of attaining its object and involving less irritation of the wound, and also (if carbolic acid be the antiseptic used) much less risk of carbolic poisoning. At the same time it must be distinctly borne in mind that the spray is, beyond all question, the least important of our antiseptic means, and that the circumstance that a surgeon does not happen to have a spray-producer at hand is no excuse whatever for his abandoning the attempt to obtain aseptic results. But if the apparatus for the spray is at my disposal, I for my part do not as yet dare to abandon it. By the careful use of our present means, the spray included, we have arrived, I think I may venture to say, at absolute security of attaining the great object in view, provided that we have the two essential conditions complied with: an unbroken skin to start with, and the seat of operation sufficiently distant from any source of putrefaction to admit of adequate overlapping of the surrounding integument by the requisite dressing. I leave it those who have done me the honour to visit my wards to judge whether I am guilty of exaggeration in making this strong statement. Such being the case, I should not feel justified, except on perfectly established grounds in omitting any part of the machinery by which results so important to our fellow-creatures have been arrived at. Nevertheless I am aware that, concomitantly with the perfecting of the spray, there has been an improvement in other parts of our antiseptic arrangements, and I am not prepared to say that our increased uniformity of good results may not be due to the latter rather than to the former. And it may be, for aught I know, that when the International Medical Congress next meets I shall be able to speak of results of a still higher order obtained without using the spray at all. For if further investigation should confirm the conclusion to which our recent facts seem to point, and it should indeed be proved that all idea of atmospheric contamination of our wounds during operations may be thrown to the winds, then no one will say with more joy than myself, "Fort mit dem Spray." The fact that normal serum is not made to putrefy by the addition of a small quantity of water came out unexpectedly in the course of an experiment designed to illustrate a view which I had long entertained as an inference from circumstances observed in surgical practice—viz., that an undisturbed blood-clot has a special power of preventing the development of septic bacteria. This property I ascribed to the white corpuscles, which are well known to retain their life long after blood has been shed from the body. I supposed these living elements of the clot to produce this effect in accordance with a principle which I believe I happened to be the first to demonstrate, but which is now generally admitted —viz., that the tissues of a healthy living body have a power of counteracting the energies of bacteria in their vicinity and preventing their development. Thus the experiment affords very striking proof of the power of an undisturbed healthy coagulum in the vicinity of living tissues to resist the development of putrefactive bacteria, even when present in a highly concentrated form, while we have, as I believe, an explanation of this power in the multitude of new living elements with which the clot was peopled. Our principle seems to find its widest application in aiding to explain the possibility of union by the first intention without the use of any antiseptic means, nay, in spite of the application of septic ones. For such is, in truth, the apparently cleanly water-dressing. As certainly as we remove it from a wound after the lapse of twenty-four hours, do we find that the diluted blood serum which then soaks the lint has a putrid smell, implying that it contains septic ferments such as would assuredly act effectively upon blood outside the body. And yet, in the not uncommon case of the occurrence of primary union under such treatment, putrefaction fails to spread into the wound; for if it did so it would inevitably provoke suppuration. Under strict antiseptic treatment union by first intention has no longer the importance it used to possess. (Prof. Lister, p. 117.)

Though the method of operating under carbolic spray very generally prevents septic effects, yet there can be no doubt that it occasionally fails where every known precaution has been taken; and, in the many cases where it succeeds, the good results may be effected in one of two ways, or by a combination of both. First, the carbolic acid in the spray may destroy the vitality of the septic germs by contact in the air; or, secondly, the particles of spray may attach themselves to the germ-particles in the air, and, when the spray-stream is properly directed, carry them over the wound, and thus filter or sweep the air, as it were, around; or, thirdly, the spray may, if so directed, sweep down the germ-particles in and upon the wound, where they may become destroyed by the carbolic lotion. In order to test the vitality of bacteria in carbolic acid, I put two drachms of decomposing urine, full of these interesting creatures, into an ounce of carbolic lotion of the strength of 1 to 80; and other two drachms of the same urine into lotion of the strength of 1 to 40. After twenty-four hours, I found these little creatures as active and lively as ever in both solutions. The result of this experiment, and the mysterious failure in some carefully conducted cases, make me suspect that carbolic spray during an operation may probably act beneficially, not by killing the septic germs, but rather by sweeping these germs from the air, and carrying them away from the wound. When we recall the results of experiments, this *modus operandi* would appear to be quite possible, if not even probable; and further, may not some of the mysterious failures have been owing to the spray having been directed at and not over the wound, which would in this case become crowded by septic germs? and we have just seen that bacteria, at any rate, are not always killed by contact with carbolic lotion. Should this reasoning not impress others as it does me, I trust that surgeons may at least have some regard to this point, and try the results of a non-poisonous spray, instead of the carbolic lotion commonly used. For this purpose, I would suggest a mixture of glycerine and water. When these are mixed in the proportion of 1 to 16, I find the specific gravity is 20 above that of water; so that, if such a lotion were sprayed across or over, and not at a wound, the result, I trust, may be even more successful than carbolic lotion has proved to be. In any case it is worth a trial, as the result would be interesting. (Dr. W. Main, British Medical Journal, Dec. 24.)

Modified Listerism.—The late Mr. Callender's method of wound-dressing was to have carbolic acid and olive oil side by side in the ward, to be mixed in their right proportion (in his wards about 1 to 12), a few minutes only before the dressing was applied. His cases almost always did well, as his statistics in the hospital reports will show. I think the disuse of carbolised oil dressing in hospitals is mainly due to the fact that the mixture is sent up to the wards in large bottles from the dispensary, and kept there till used up, sometimes for weeks or even months. If any person will compare the appearance and smell, especially the latter, of a freshly made sample, with those of one that has been kept for a month or more, he will at once be aware of a great difference; and if he will further make a trial, by treating similar cases with the two sorts, he will be convinced of the deterioration of the mixture in antiseptic qualities after being kept. (Dr. F. M. Pope, p. 134.)

In a paper of mine on "Surgical Dressings in Private Practice," published in a contemporary, is recorded a case of femoral hernia treated according to Mr. Gamgee's plan with very satisfactory results, and in speaking of this mode of treatment I observe: "His dictum of rest, drainage and pressure, if duly carried out, will save much time and trouble, and show results comparable with the antiseptic method. Absorbent wool made antiseptic by means of carbolic, salicylic, or boracic acid, or by terebene, is much nicer as a dressing to a tender wound than either tenax or carbolic tow, but in this mode of treatment any of these form absorbent dressings." In my cases lately at the Wigan infirmary I have tried salicylic silk as recommended by Mr. McGill of Leeds, and can also speak of this as a nice light dressing in surgical cases. (Mr. W. Berry, Lancet, April 15.)

Simplified Antiseptic Dressing.—Prof. Little of the University of the City of New York, has, during the last six years, had recourse to a simplified form of dressing with satisfactory results. He washes the wound in a solution of carbolic acid of one to twenty, and then covers the parts with a thick layer of borated cotton (much of this is made with a solution of borax instead of boracic acid; made with this last, the cotton on being burned gives out a bright green colour, little of which is seen when borax has been employed), over which a gauze bandage is snugly and evenly applied. At first he used bandages made of antiseptic gauze, but for the past three years he has had them made of plain, uncarbolised cheese cloth, these thin bandages distributing the pressure more evenly over the cotton, and being more easily saturated with fluids than those of unbleached muslin. The patient is told to keep the outside of the bandage wet with carbolic acid solution at 1 per cent. The parts are kept at rest, and the dressings may be left undisturbed for several days, unless there is pain, rise of temperature, or discharge through the dressings. The patient should be instructed to loosen the bandage at once if any pain occur. (Med. Times and Gazette, Jan. 14.)

The value of cotton-wool as an antiseptic dressing is, I think, not sufficiently appreciated by the profession. Used in the way I have indicated, it seems to me to be as perfect an antiseptic dressing as the gauze and other materials recommended by Prof. Lister, while it is free from the objections that pertain to this, and which materially hinder their use by the general practitioner. If applied in sufficient quantities around an open wound, it protects this from the "floating matter of the air," which is supposed to be the real incitor of suppuration. It is the best germ-filter known to us. I have used the very excellent borated cotton made by Mr. am Ende, of Hoboken, containing 15 per cent. of boracic acid. Keeping it wet externally with the solution of carbolic acid renders it more surely antiseptic. To insure success in the cases in which this dressing is used, full precautions as to rendering the instruments, sponges, and the hands of the surgeon aseptic, and the use of drainage-tubes if necessary, should not be neglected. Catgut or torsion should be used to arrest hemorrhage. The spray may be resorted to, if thought necessary, at the second dressing. I now usually apply carbolised oil, of the strength of one to twelve, to the wound, to facilitate the removal of the cotton, which is otherwise apt to adhere after the first dressing. I would state, in conclusion, that my experience thus far seems to show that this simple dressing, so easy of application, is as thoroughly antiseptic as Prof. Lister's appliances, and that it has the very decided advantage of doing away with the necessity for using costly 'protective-oil-silk,' 'mackintosh cloth,' 'carbolised gauze,' &c., and gives us a dressing that can be used by anyone under any circumstances, be it in city or country. The borated cotton is easily kept for months unchanged. The fact that the dressing need not be done oftener than once in several days will especially commend it to the country practitioner. (Prof. Little, Med. Times and Gaz., Jan. 14.)

Iodoform Cotton.—Iodoform cotton is an introduction from Germany; and consists of absorbent cotton-wool which has been thoroughly impregnated by means (we believe) of soaking it with an etherial solution of the drug. An absorbent lint has been prepared in the same way. This may be advantageously applied to a variety of wounds and sores; but its efficacy is particularly manifested in wounds about the perineum, say, *e.g.*, a hernia. Thus the operation may be performed with the strictest antiseptic precautions, but instead of putting on a gauze dressing, the parts are enveloped in a mass of the cotton, a wise precaution being to previously smear the surrounding hairs with some iodoform ointment. If the stitches are of catgut the dressing may be left on for a week, at the end of which time the drainage-tube may be removed; the stitches, if they have not become absorbed in their deeper parts, may be either taken away or left, as desired, and the second (which will probably be the final) dressing applied. It cannot fail to be observed that this greatly increases the possible field for the performance of antiseptic operations in the country. (Ed. Medical Times and Gazette, Jan. 14.)

Bow Legs.—*New Splint.*—This is an apparatus, which consists of a rigid steel rod, fixed above to a thigh-piece made of thin copper shaped to the form of the thigh, and attached below to a foot-piece having a box on it to permit the rod to slide in and allow for any increase in the length of the tibia as it becomes straight. Sliding on this rod, so that pressure can be applied at any point, is a tourniquet screw, having a short leather splint attached to it by means of straps and buckles. This leather splint is placed over the curved portion of the tibia, and, when the screw is tightened, can be made to act very powerfully on it. The rod forms the inner splint and also the fulcrum for the screw, and being slightly convex—the concavity being towards the leg to which it is applied—it gives a better leverage, and prevents the skin from being pressed on. The splints are applied in the following manner:—The foot and thigh-pieces are attached to the limb by broad strips of adhesive plaster; then a plaster of Paris bandage is applied over the foot-piece, and twisted as it is passed over the box, so as to allow the rod to slide freely. The plaster of Paris is next applied over the thigh-piece. The leather splint, well padded, is then placed over the curved portion of the tibia and secured to the screw by the buckles. The buckles should lie on the leather so as not to press on the skin. Pressure might now be made, but it seems better to wait until the plaster of Paris becomes perfectly hard. (Mr. G. R. Gilruth, p. 148.)

Diseased Bursa Patella.—Probably the best plan is to tap the bursa with a small trocar, and apply a blister over it, keeping the leg at rest on a back splint. As soon as the blistered surface will bear it, the knee should be strapped so as to induce the walls of the sac to cohere. Should this proceeding fail, and the fluid reaccumulate, the bursa may be again tapped, and a teaspoonful of strong iodine tincture thrown in, just as one does with a hydrocele. But if the tapping and injecting, the blistering and strapping all fail, what is to be done? My present practice is to *excise the bursa* at once. In the method of operating I have ventured to make an innovation upon the ordinary practice of removal by a median incision. I have for some years made two incisions, one on each side of the joint midway between the edge of the patella and the femoral condyle. There seems to be two advantages which this method possesses. In place of there being after the operation a median cicatrix, which is painful to kneel upon, the skin in front of the patella is as sound as ever. This is no small advantage, seeing that the persons who get these bursal tumours generally make their living by kneeling, and have to take to kneeling again, even after they are removed. While removing a bursa by the double incision, the great point is to keep steadily cutting upon the tumour itself. In front of the patella no possible mischief can be done, but just at its edge the capsule of the knee-joint is very thin indeed, and here there is a strong tendency to get off the tumour and go cutting downwards through the aponeurotic capsule. (Mr. W. M. Banks, p. 153.)

On reading Prof. Lister's lecture "On the Relation of Micro-organisms to Inflammation," I see that he speaks of the treatment of *chronic* inflammation of the bursa patellæ by means of puncture and the introduction of a drainage-tube under antiseptic precautions. The antiseptic dressing he says has to be changed in a day or two, and the tube shortened. At the next dressing, as there is usually only slight oozing, the tube is altogether dispensed with, and in a week or ten days the puncture is healed. Now, with all due deference to so eminent an authority, I venture to question whether such elaborate precautions are really required. I began puncturing cases of inflamed bursa patellæ (both acute and chronic) upwards of ten years ago, with an ordinary trocar and canula, and though I have never used the precautions which Prof. Lister suggests, I have never had a single case turn out badly, and the time required to effect a cure is from a week to ten days. All I do is to squeeze out the contents of the sac (of course taking care that no air enters), and before relaxing the pressure put a piece of strapping over the puncture. The leg has to be kept quite straight by means of a wooden splint placed at the back of the knee and three pieces of strapping, one across the patella and one above and below. Sometimes I cover the knee with blue ointment spread on lint, but to this I do not always resort. Occasionally, after three or four days, a second puncture has to be made owing to fresh accumulation, but this is by no means a rule. This treatment saves the necessity of evaporating lotions and the internal use of iodide of potassium, besides lessening very materially the duration of the case. (Mr. F. P. Atkinson, Lancet, Nov. 26.)

DISLOCATION OF THE RIGHT SHOULDER JOINT.—I place myself on the same level as the patient, and on his right side, but in front of the limb, raised to a right angle, or higher or lower as I find most convenient, the elbow also being bent to nearly a right angle, and allowing the latter to rest on my left shoulder or upper arm, and using my left hand to steady the upper part of the arm, or control the scapula, if necessary, I insert the tips of the fingers of my right hand into the axilla beneath, and to the inner and anterior or posterior part—according to the nature of the dislocation—of the head of the humerus, and by gradually increasing pressure, lift it quietly into the glenoid cavity, where I generally found it will go without much difficulty. If the bone get jammed at all or the patient is very muscular, a slight amount of traction, with or without a little rotary motion, or manipulation of the scapula with the left hand, I find will almost entirely overcome the difficulty; and the pressure on the head of the bone being continued, it will slip into its place. On the left side the treatment is just the same, the opposite hands, of course, being brought into use. Dislocations of the elbow-joint I find most easy to reduce. I merely bend the patient's forearm on the arm to a right angle, or angle of 120°, and allow his forearm to rest on my right forearm in the case of a right dislocation, or on my left forearm in the case of a left dislocation; or I support the forearm with the inner fingers of both hands; and with the other fingers applied on the highest point of the bone dislocated upwards and backwards, and the thumbs on the lowest point of the humerus dislocated downwards and forwards, press the bones into their proper places. This dislocation can often be reduced by one hand only. (Dr. J. H. Anderson, p. 146.)

Excision of the Knee-Joint in Early Life.—1. Excision of the knee should not be looked upon as a last resource, but should be undertaken, if possible, before any profound organic changes take place. 2. Expectant treatment, to be efficient, must be undertaken at an early stage of the disease, and extend over a period of at least two years. 3. No better result than ankylosis can be looked for by this method. 4. In a patient with a predisposition to secondary tuberculous developments, the possibility of the recurrence of the disease after expectant treatment must be borne in mind. 5. In cases attended with prolonged suppuration, the chances of the occurrence of visceral, especially renal disease, must not be lost sight of. 6. Where the skin is unbroken, the disease limited, an efficient method of fixation applied, and a rigid system of antiseptic dressing of the wound adopted, primary union may, in the majority of cases, be anticipated. 7. When these latter conditions are fulfilled, excision of the knee-joint cannot be longer regarded as the formidable procedure it was formerly held to be. 8. The alleged unfavourable results of excision of the knee-joint in early life are opposed to more extended clinical experience. (Dr. W. Stokes, British Medical Journal, Dec. 10.)

Excision of the Tongue.—*Whitehead's Operation.*—Whitehead's operation consists of cutting through the attachments of the tongue, by successive short cuts, with a curved pair of scissors, the organ being drawn forwards and upwards by means of stout whipcord. There cannot be a doubt about the comparative simplicity of this operation in excision of the whole tongue. By means of the scissors one can make absolutely sure of cutting away the entire organ; a point which is not so certain or easy of attainment by means of the écraseur or galvano-cautery. Its advantage over ligation is self-evident. Although my experience of Whitehead's operation has been confined to this case, still the danger from the bleeding seems to me to be greater than he is willing to admit, and on repeating the operation I shall first ligate the lingual arteries as recommended by Billroth, assured that in doing so the danger to the patient is minimised. (Dr. Elder, p. 182.)

Fracture of the Patella.—The treatment which I primarily adopt for simple transverse fractures of the patella is that which is in common use at the Middlesex Hospital, and which certainly yields very good results: it is the application of continuous extension to the upper fragment of the bone by means of an india-rubber multiplier. On admission the leg is placed on a well-padded McIntyre splint, and with a pad of lint beneath the lower edge of the fractured patella, the leg is bandaged up to the knee. Another pad of lint is then laid across the upper end of the broken bone, and a broad piece of moleskin plaster cut like a horse-shoe, with its ends prolonged, is next laid on the thigh, so that its curved edge rests on the pad of lint, and its ends extend below the joint. A few turns of a bandage are then applied around the thigh, to prevent the plaster slipping. To the ends of the plaster are sewn tapes, which are attached to an india-rubber multiplier on each side of the leg; and from the further ends of the multipliers are also tapes, which are fastened tightly behind the footboard of the splint. The thigh is then bandaged to the splint, which is raised on a slightly inclined plane. In this manner a continuous extension is kept on the upper fragment of the patella. The splint is kept on for at least six, and frequently eight, or even ten weeks. After the splint has been removed the leg is placed in a plaster-of-Paris bandage, or in a long leather splint, and in another week or fortnight the patient is allowed to leave the hospital, but with instructions to return from time to time to have the plaster-of-Paris bandage reapplied. After the plaster bandage is given up, the patient should have a patella splint with side irons and a check-joint at the knee, so as to limit to the smallest degree the flexion of the knee. This splint he should continue to wear for at least six or eight months, so that he will not attempt to flex the knee-joint as completely as is required for walking until from twelve to fourteen months from the date of the accident. (Mr. George Lawson, Lancet, Dec. 17.)

Cause of Separation of Fragments in Fracture of the Patella.—The separation of the fragments is not caused by the muscles. Repeated observations have convinced me that it is always caused by, and in proportion with, the effusion into the joint. If there be no effusion, there is no separation. The muscle is not a piece of India-rubber to contract, and remain contracted, as soon as one end is loosened. It is as easily capable of relaxation as it is of contraction, and, when the limb is at rest, it is always relaxed. When relaxed, there is no reason why the upper fragment of the broken bone should not come easily down to the other; and, in point of fact, in cases where there is no effusion, it does do so. I have demonstrated this repeatedly. You may then clear your minds of all misconceptions as to spasm of the muscle being the cause of persisting separation. Such spasm may, of course, cause separation at the moment of the accident; but, as soon as the limb is at rest in bed, its agency ends. I repeat, you will always find the difficulty of approximation in proportion to the amount of swelling. (Mr. J. Hutchinson, p. 138.)

HIP AFFECTIONS IN CHILDREN.—*The Double Splint.*—Mr. Bryant's double splint for affections of the hip in children can only be understood by reference to the description of it. When once the little one is tightly fixed in, there is a very poor chance of his getting loose. Indeed it is absolutely impossible for him to produce any movement save that of the arm, head, neck, and a slight tilting of the pelvis, which is possible in the dorsal decubitus. So that not only is the joint itself practically immovable, but any jar or shock from the movement of other parts prevented. Then, as to the possibility of moving the patient for dressing or cleaning purposes without causing movement of the joint or pain to the patient. This is admirably affected by the splint; and had it no other claim for recognition, I think this feature alone would prove sufficient to ensure it. The patient can be easily lifted from one bed to another, or from one room to another. With one hand beneath the buttock and one beneath the back, movement is easily effected. Again, the patient can be readily turned on to his side, thus giving convenient access to bed sores and to wounds, whether the result of the disease or the sequence of operation. All this can be easily effected without the least pain to the patient, and with the happy assurance that the natural process of repair is being in no way impeded or interfered with. The freedom from pain is frequently well marked. The little patients, when entering the hospital, cry at the least attempt at movement of the limb, but after a day or two in the splint, the acute sensitiveness of the joint subsides; and though there is at first a slight objection to so formidable a companion, they soon get used to it, and gain an affection for it from the security which they feel in its wooden embrace. (Mr. A. E. Maylard, p. 142.)

LIGATURES.—*The Catgut Ligature.*—It is little more than a year ago that Mr. Lister delivered his presidential address to the Clinical Society of London on the catgut ligature. In it he described a mode of preparing the catgut by soaking it in a watery solution of carbolic and chromic acids, whereby it was strengthened and in other ways adapted to the purpose of tying arteries in continuity. On Friday last he declared that, after delivering the address referred to, and which he believed to include the final results of his experiments, he was disappointed at discovering that ligatures prepared in the way recommended varied a good deal, the thicker ones becoming speedily infiltrated at the surface in place of being eroded. Further inquiries and careful experiments led him to associate the faults complained of with details of manufacture of the catgut. Following out the line of experiment suggested in this connection, he finally secured a catgut free from the ill qualities which made the chromicised substance of less value. This desirable end is attained by steeping the catgut, first in a solution of chromic acid, 1 per cent., for twelve hours, and subsequently in sulphurous acid, B.P. strength, for the same length of time. The material thus prepared can be stored in the dry state until required for use, thus doing away with the need for keeping in carbolised oil, as must be done in the case of the chromicised gut previously described by Mr. Lister. The new ligature before using is plunged into a watery solution of carbolic acid, which can be kept on the tray containing the instruments; and its inventor claims for it that the infiltration observed in the case of other catgut ligatures does not occur with it, and that it is eminently strong and adapted to the purpose of tying vessels. (Ed. Medical Press and Circular, March 15.)

A very thick catgut ligature to an ordinary-sized vessel will probably fail, whereas a small or medium-sized thread will ensure a satisfactory result. To those who are in a position to carry out experiments I venture to suggest the advisability of determining whether, in many such cases, the secondary hemorrhage is not chiefly due to the employment of catgut of too large a size. Prof. Lister tells us that "for tying an arterial trunk in its continuity, catgut as thick, when dry, as ordinary purse silk will be found best. But for ordinary wounds, where, if one ligature happens to break, another can be easily applied, much finer kinds may be employed, and are convenient from their smaller bulk." To this I would add, that when an artery has been successfully tied with the finer kinds of catgut the result will be more satisfactory than if thick strands had been used. (Dr. J. G. French, p. 167.)

Ox-Aorta Ligatures.—Mr. Barwell, who has used ligatures made from the ox-aorta in cases of aneurism of the aorta, says:—I am able to report fourteen deligations, which, being varied, are more than sufficient to prove the solubility of the ligature which I introduced two years ago, and also to show that it is not absorbed too soon. Hitherto no instance of tying either the innominate or the first part of the subclavian has presented itself. I would wish, however, to repeat that the deligation of such large vessels so near the heart can only be safely undertaken with a ligature which leaves all the arterial tunics entire and unwounded, a result only to be obtained by using a flat or ribbon-like band. The extensor or suspensory tendon of the kangaroo's tail, tendons from the whale, decalcified bone from quadrupeds and birds, have all been introduced into the human body, and have become absorbed or incorporated with the tissues. The (fibrous) structure which intervenes between the mucous and muscular coat of the sheep's intestine, the substance of which catgut is made, would doubtless act constantly and always in the same way were it not that a certain, or rather an uncertain, amount of putrefaction is essential in the primary manufacture; it is this that renders a long and elaborate preparation necessary, and the ultimate result uncertain. To me the important points appear to be that the substance used should be absolutely free of all taint of decomposition, and that it should be cleansed from any accidental impurity by a short immersion in some antiseptic fluid. If these essentials be secured, the choice of particular substance is immaterial; sufficient strength, persistency, ability to keep a safe knot, as also facility of acquisition, are the main considerations for an ordinary ligature. But for safe deligation of the larger arteries in continuity, a flat form such as will not divide the tunics is a great desideratum. (Mr R. Barwell, p. 164.)

New Material for Splints.—*Corrugated Paper.*—The material in question is known as "corrugated paper;" is made by George Smyth, 47, Arlington Street; and is merely an application of the principle of giving strength, by fluting, to a material not inherently stiff, as one sees is effected in a porcupine's quill, or the feathers of a bird, or in the corrugated iron chapel. It consists of two pieces of very stout brown paper — one flat, and the other moulded into ridges and furrows. I may mention that the commercial use to which this paper is put is, I believe, principally that of either packing wine bottles in cases, or else of enabling single bottles to be wrapped up into a parcel, which shall have no external suggestion of the nature of its contents about it. (Mr. Walter Pye, p. 147.)

Medullo-Arthritis.—Chronic joint inflammations have long been called strumous, and I believe with justice. One commences as an inflammation of the synovial membrane, the other as an inflammation of the pink marrow in the cancellated ends of the long bones; and both ultimately proceed to disintegration of all the structures entering into the articulation. The first might conveniently be called "synovio-arthritis," the second "medullo-arthritis." In medullo-arthritis give a free exit to the suppurating marrow bound down inside the femur, and engaged in the meshes of its cancellous bone. The only other alternative is amputation. In one case six ounces of flaky pus were removed from the right knee-joint, and two large drainage-tubes were inserted. On the left limb an opening was made by a gouge towards the outer and posterior aspect of the outer condyle, and the contents of both condyles were scraped out. Sufficient proof of the existence of disease was given in the fact that a Volkmann's spoon was strong enough to break down the cancellous bone inside. With the little finger in the opening it was possible to feel the articular cartilage. A large drainage-tube was inserted into the bottom of the cavity in the condyles. From that time to the present, when you see the patient jump down from a chair and walk about without lameness, the course was one of uninterrupted improvement. In another case the whole of the contents of the femoral condyles were removed through an opening made (in this case) on the inner side. After all the softened tissues had been removed, the finger introduced through the opening not only could feel the cap of the articular cartilage, but could make it pout at the sides of the ligamentum patellæ. A large drainage-tube was inserted and retained for six weeks. At the end of that time it was removed; antiseptics were stopped and boracic lint dressing applied. There is still some slight discharge, but this is daily diminishing, and the patient will soon, I hope, be walking about. Here the result will not be so good as in the other case, on account of the adhesions which have formed. I did not dare to institute passive movement here, because the articular cartilage was left with so little support. And as the cartilage will give us some of the new bone destined to fill up the cavity which we made, we must be careful not to interfere with its nutrition more than we can help. Besides, I believe it had contracted adhesions to the tibial cartilage during the period the joint was being drained. It is an opprobrium to our art that we should so frequently have to stand by with folded hands while such cases doggedly go on to that extent which will justify the major operation. I believe that if the pathological condition is recognised sufficiently early the progress of the disease might be nipped in the bud. If it is an area of inflamed marrow that is the root of the evil, and if we can reach and remove this, we ought to cure the disease. If there be any justification for the treatment of strumous glands in the neck by removal, there is much stronger justification for the removal of a similarly diseased structure inside bone. Inside a bony shell everything is against spontaneous cure. Compressed and perhaps strangulated by the outer shell the inflammatory products are driven to destruction; and, when decay has set in, the abundant bony trabeculæ entangle the degenerate products and delay their discharge through any opening that may have formed. The very chronicity of its course begets thoroughness of destruction. Long after the marrow is in a state of suppurative or caseous degeneration the bony trabeculæ remain to confine it, simply because, being now almost bereft of vitality, the decayed tissues cannot cause bony absorption or disintegration. Bony caries is always a tedious complaint. (Mr. J. Greig Smith, p. 156.)

Plaster-of-Paris Bandages.—*New Mode of Applying.*—The material employed is a coarse-meshed muslin (tarlatane in France, crinoline or coarse book-muslin in our drapers' shops at home); costing about 3d. per yard. A piece of this of sufficient length is taken and folded into a long bandage, having from sixteen to twenty-four thicknesses of the material, according to the requirement of the special case, and varying in width from two to three fingers' breadth. It is desirable, the folds being so numerous, that a loose thread tacking should be run along the free edges, to preserve the shape of the bandage and provide against unfolding. Before proceeding further, the bandage should be soaked for a few minutes in water, so as to remove the stiffening or glaze which is liberally introduced in the process of manufacture of the material. The plaster-of-Paris should be mixed with water (an equal bulk of each seems to be the most satisfactory proportion), in a large, rather shallow, bowl. As the mixing proceeds, the finely powdered plaster should be dredged on the surface of the water until it is exhausted. A mixture of creamy consistence will be the result. Into this mixture the bandage, prepared as above, must be placed, and quietly moulded or kneaded, so that every part of it may be thoroughly permeated with the compound. It may be very loosely rolled up from each end towards the centre, for convenience of using; but there must be no attempt at compression or squeezing of the rolls as this is done. The desideratum is to have a bandage of very definite thickness, thoroughly and equally saturated with liquid plaster. (Dr. W. Newman, p. 149.)

AFFECTIONS OF THE SKIN, ETC.

BURNS.—*Iodoform.*—An extensive stinking burn may be purified by a single application of the powder; we have ourselves employed it in such cases with the greatest possible benefit, and it may be remarked that if it be intended to dress the burn with protective and boracic lint (a most excellent application in such cases), the use of the iodoform gives this great safeguard, that supposing a spot of putrefaction be left beneath the protective, or putrefaction spread inwards beneath the edge at the part from which the greater part of the discharge escapes, the mischief does not extend itself, but is limited or subdued by the iodoform in its neighbourhood. (Ed. Medical Times and Gazette, Jan. 14.)

Soda a Remedy for Burns and Scalds.—The author in 1844, whilst engaged in some investigations as to the qualities and effects of the alkalies in inflammations of the skin, &c., was fortunate enough to discover that a saline lotion, or *saturated* solution of the bi-carbonated soda in either plain water or camphorated water, if applied speedily, or as soon as possible, to a burned or scalded part, was most effectual in immediately relieving the acute burning pain; and when the burn was only superficial, or not severe, removing all pain in the course of a very short time; having also the very great advantage of cleanliness, and, if applied at once, of preventing the usual consequences—a painful blistering of the skin, separation of the epidermis, and perhaps more or less of suppuration. For this purpose, all that is necessary is to cut a piece of lint, or old soft rag, or even thick blotting-paper, of a size sufficient to cover the burned or scalded parts, and to keep it constantly well wetted with the sodaic lotion so as to prevent its drying. By this means, it usually happens that all pain ceases in from a quarter to half an hour, or even in much less time. (Mr. F. Peppercorn, p. 251.)

CUTANEOUS PRURITUS. — *Succus Senecionis Jacobææ.*—Purdon believes that general itching of the cutaneous surface, when unassociated with any affection of the skin, is due, as a rule, to deranged action of the liver connected with the abuse or excessive use of alcoholic liquours, or from jaundice. As a hepatic stimulant in such cases he recommends "rag-weed," an excellent preparation of which is made by Grattan and Co., Belfast, and called by them Succus Senecionis Jacobææ. The dose is from one teaspoonful to a couple of tablespoonfuls, either with or without a teaspoonful of sulphur, early in the morning. Locally, the Turkish bath and sponging the body with vinegar and borax is useful, and relieves the attack for several hours. (Practitioner, Jan.)

ECZEMA.—When there is much clear serous effusion, and especially in eczema of the folds of the limbs, powders are often better than either lotions or unguents. Finely powdered chalk, oxide of zinc and starch, dry up such weeping surfaces and form a false scab under which the healing process goes rapidly on. The intertrigo of infants and of the breasts in women is often quickly relieved by first cleansing with white of egg, carefully drying, and then powdering with oxide of zinc. This plan is, however, ill adapted when the affected parts are allowed to be in motion, as with intertrigo of the nates. In such cases the ordinary benzoated zinc ointment, with double the officinal quantity of zinc oxide so as to make it drier and firmer, or vaseline with zinc, are better applications; they should be spread upon thin rags and kept in place with a suspensory bandage between the thighs. When a similar condition arises from riding or rowing, the interval between the periods of irritation are longer, and it is possible, by scrupulous cleanliness and diligent powdering, to procure healing without altogether stopping the cause. There are, however, some cases of eczema, especially in the young, and of the moister kinds, which resent every kind of medication, and can only be treated by the sedulous employment of the most soothing and indifferent applications. Among these, as I have already pointed out, water cannot be reckoned; it almost always does harm, and alkaline washes are worse. Thin size, cold cream, or vaseline are the most likely to succeed in such cases. Glycerine of starch occasionally succeeds when everything else seems to fail, but it often proves extremely irritating, and on the whole is less used than it was some years ago. Glycerine has the advantage of mixing freely with water, and may thus be used as a vehicle of tannin or of borax to moist surfaces and mucous membranes; but the withdrawal of water from the surface appears in itself to be sometimes an irritant. After the acute stage of an eczema has passed, and it is as a chronic inflammation that it usually comes before us, astringents are still indicated, but instead of soothing and protection some stimulation is necessary. This we obtain by adding a mercurial salt, and the ung. metallorum of our Guy's Pharmacopœia is one of the best combinations for treating impetigo and chronic eczema. The red oxide ointment, either alone or added to ung. zinci, is also very valuable, especially in the most chronic and indolent forms which approach ulceration. In obstinate eczema, especially where of small extent and moist, the application of liquor potassæ, as advised by Dr. McCall Anderson, is often an efficient and rapid means of cure. Acid nitrate of mercury is the best application for local deep fissures of inveterate eczema rimosum. In the very chronic eczema, which is dry and scaly without much active inflammation and itching, tar ointment, or our ung. liq. carbonis detergentis are indicated. For single-patch eczema, if wet, liq. potassæ; if dry, ung. picis liquidæ is best. Meantime we must prevent the irritation of scratching and rubbing by relieving its cause. Oxide of zinc, calamine, or borax, as ointments, weak corrosive sublimate wash or ung. hydrargyri ammoniata, hydrocyanic acid lotion (ʒ iv—ʒ vj to a pint), cyanide of potassium ointment (gr. ij to the ounce), and mere protection from the air by such indifferent applications as cold cream and vaseline:—these are all useful for the purpose indicated, the poisonous sedative being used with caution when the moist surface is extensive. (Dr. H. Pye-Smith, p. 240.)

Eczema of Infants.—Bantingism.—Eczema is especially common amongst infants, and particularly amongst lymphatic infants, that is to say, fat and pasty-looking infants. I do not refer only to those instances in which the very fatness of the infant is the mechanical cause of the complaint; that is to say, where a fold of skin in the fat infant becomes raw and discharging (intertrigo); but I refer to the well known fact that infants of this constitution are more liable than others to eczema (of the scalp and other parts) not coming under the head of intertrigo, and that their eczema is more profuse in its discharge, whether that discharge be serous or purulent, than it is in other infants; it is also more obstinate

To Bantingise a suckling infant is, of course, not very practicable, but a child of two or three years old can be dieted very readily. It does not appear necessary to the end in view that the diet should be restricted in quantity as well as in quality, which of course was essential under the *régime* laid down by Mr. Banting. It is simply necessary to limit the fat-producing elements of food for the amelioration of eczema in lymphatic young children. At least so I have found. By this means their excessive obesity becomes diminished and their eczema very remarkably improved within ten days of commencing the regimen, and that without any injury whatever to their general health, so far as I can judge. (Mr. Balmanno Squire, p. 248.)

Eczema Palmarum.—Dr. Lush recommends the following syrup for palmar eczema:—Bicarbonate of soda, 8 grams; bicarbonate of potash, 4 grams; neutral glycerin, 4 grams 50 centigrams; tincture of opium, 6 grams; distilled water, 192 grams. Dissolve. To be used as a lotion to allay the irritation produced by chronic eczema. In very obstinate cases, where the skin has become very brittle, Dr. Liveing recommends the following solution:—Liquor potassæ, 6 grams, distilled water 192 grams. (Practitioner, Jan.)

FACIAL ERYSIPELAS.—*Ergotine.*—I would call attention to the value of a solution of ergotine (1 in 50) as a local application in facial erysipelas. In an outbreak of that complaint which occurred in my practice a few months back, the relief to heat and pain, the reduction of swelling, and the rapid subsidence of the disease, were most remarkable. I therefore venture to suggest its trial at the hands of other members of the profession. (Dr. K. W. Millican, Brit. Med. Journ., Dec. 10.)

LICHEN PSORIASIS.—*Arsenic and Tar.*—My treatment is tar externally, and arsenic internally. In nine cases out of ten, these remedies will cure lichen psoriasis pretty quickly. Some of you may remember a man whom we had under care six months, a splendid specimen of the disease. He had been sent to me by Mr. Forshall, of Highgate. It was a first attack, and occurred to a healthy young man. I prescribed arsenic and tar. Through Mr. Forshall's kindness, I had an opportunity of seeing this man again last week. He told me that about six weeks' use of the remedies quite cured him, and that he has, during the last four months, remained without treatment quite well. In our last case, however, we have not been so fortunate. Our patient was of very peculiar nervous system, in fact almost insane, and the influence of arsenic appeared to be to excite her. Several times we had to suspend it on account of the irritable condition which it appeared to produce, and finally she was discharged from the hospital uncured, in consequence of the trouble which she gave in the ward. As a rule, I have found lichen psoriasis more easily influenced by treatment than common psoriasis. The cure is also usually more complete. The periods of immunity are also longer, often not less than several years; whereas psoriasis, however good the cure, usually relapses, I think, within the year. (Mr. J. Hutchinson, Brit. Med. Jour., Jan. 7.)

MOSQUITO-BITES.—As a protection against mosquito-bite, the natives (Ceylon) smear the skin with lime-juice. After bathing this is said to be very effectual. Various remedies have been recommended, either as prophylactics or as affording relief, such as cocoa-nut oil, carbolic oil, solution of ammonia, &c., but the application I have found most effectual is to smear the hands, where bitten, with a moist cake of soap, and allow the thin lather to dry into the skin. I have frequently been obliged to resort to this for relief, and have found that all itching and pain disappeared in ten or twelve minutes after the application was made, and did not again return. Besides being effectual, it has the advantage of being always at hand and easy to use. (Dr. J. Stevenson, Edinburgh Med. Jour., Feb.)

PEMPHIGUS.—*Arsenic.*—A patient was covered from head to foot with bullæ; the trunk was less severely affected than his limbs, head, and genitals; on these, there was nowhere a space as large as the palm free from bullæ, and on the trunk also there was a considerable number. He was in a miserable condition from pain and irritation. The eruption had been out about ten days, and it affected the mucous membrane of his mouth as well as the skin. You may remember that we kept him in bed for a few days before we used the magician's wand, in order that all might see that there was no natural tendency to amelioration. More bullæ came out; then, without making the slightest change in diet, we ordered a few drops of a tasteless solution of arsenic, to be swallowed three times a day. The result was that, at our next visit, most of the bullæ had dried, and there were no fresh ones. He continued to improve greatly for ten days, when suddenly a few fresh small bullæ seemed to threaten a relapse. We doubled the dose of our remedy, making the dose eight instead of four drops; and, from that day, with the most trifling exception, the recovery has been uninterrupted. With such a fact before you, let me beg of you to believe in drugs, and to treat empiricism with respect. In the prescription which I ordered, I availed myself solely of empirical knowledge; I prescribed, just as any old woman might prescribe, that which I knew would do good. Concerning the nature of pemphigus, I knew nothing; of its cause, absolutely nothing; of its clinical relationship, but little; of the *modus operandi* of arsenic, I knew scarcely more; but this I did know as a fragment of assured conviction, that arsenic would cause the pemphigus eruption to disappear, and the patient to regain his health. Far be it from me to speak slightingly of scientific work; let us by all means work as hard as we can in the laboratory and microscope-room, and penetrate as far as we possibly can into the mysteries of disease; let us never weary in our search after causes, or in our endeavour to find practical application for the facts of physiology. But, whilst doing this, let us remember that, as regards the relief of suffering, much of our usefulness must be based upon knowledge which is nowise scientific, but simply a matter of experience and memory. We have many specifics for many maladies, or rather for many symptoms, and he is the most successful practitioner who has stored in his memory the largest number of them. As years go on, we shall add many more to our list; and I doubt not that there are those who now listen to me who are destined to give help in their discovery; for discoveries in this direction are rarely made by single observers, but rather by the concurrent work of many experimenters, all keeping their eyes open, willing to try new things, and resolute to store faithfully the results of their observations. Iodide of potassium for tertiary syphilis, the bromide for epilepsy and as an anaphrodisiac, iodoform for phagedena and specific ulceration, balsam of Peru for scabies; so silently have these invaluable specifics been introduced into practice, that it would puzzle most of us to say who first recommended them. I mention this fact, in order to show how important is the honest labour of all in the pursuit of therapeutics. We all prescribe, and we ought all, on system, to observe and record the results of our observation as to the effect of drugs. Five-and-twenty years ago, I believe that the case of pemphigus which you have seen cured would have been found incurable in all the medical institutions of the world, with one single exception. Much more recently than that, the disease was pronounced by Hebra to be invariably fatal. So, indeed, it would have been to this day, if we had not found out arsenic. I know of nothing else that will cure it. Our patient was already beginning to emaciate, and, in the course of a few months—possibly of a few weeks—he would have had to die, worn out by the constant discharge from his skin, had we not put the arsenic into his blood.

Never shall I forget seeing a poor wretched child carried on a bed into Mr. Startin's out-patient room at the Blackfriars Hospital for Skin-Diseases. It had been brought straight from the wards of one of our largest hospitals, where, during three months, all had been done for its help that benevolence, aided by the science of the day, could suggest. Yet it was emaciated to skin and bone, and so covered with sores, that it was impossible to put its clothes on. A few minims of arsenic were prescribed, and in a few weeks the child was well. So much for empirical knowledge; so much for drug-specifics. (Mr. J. Hutchinson, Brit. Med. Journ., Jan. 7.)

The idea undoubtedly conveyed by Mr. Hutchinson in his graphic description of the cure of pemphigus, by arsenic is, that pemphigus is invariably amenable to a course of arsenic. This I most certainly dispute. Were the disease, in all cases, due to "simple" constitutional causes, then, no doubt, arsenic would be an infallible remedy; but, seeing this is not the case, the logical inference must follow that it is not. That pemphigus is frequently connected with the syphilitic taint, no one, I think, acquainted with syphilis will gainsay. In such a case, arsenic could be of no avail; and the only remedy would be to remove the cause, viz., the syphilitic taint. A case of this kind, treated by my much respected teacher Prof. McCall Anderson, made upon me, as a student, an indelible impression. This case was a marked one, the body being covered with bullæ from the crown of the head to the sole of the foot, and the patient himself in an extremely weak condition. Acute inquiry and observation left no doubt on the mind of Prof. Anderson that the disease was of syphilitic origin; and that his diagnosis was correct was most satisfactorily proved by the anti-syphilitic treatment adopted. In the course of a few weeks, by the hypodermic injection of a solution of the bichloride of mercury, this man was perfectly well. That it was syphilitic in origin there could be no doubt, as, had it not been so, the mercury, in his weak constitutional state, would have been sure to make him feel worse, whereas the patient expressed himself daily as being "much better." Seeing that pemphigus, where for some reason or other the system has become reduced below par, is not an uncommon form of cutaneous disease in connection with syphilis, it would be well to bear in mind, I think, that such an anomaly exists, requiring treatment *per se.* (Dr. John S. Main, British Medical Journal, Jan. 21.)

PRURITUS VULVÆ.—Pruritus vulvæ, as is well known, is an exceedingly obstinate affection. Dr. Besnier has successfully treated a patient whose pruritus had previously resisted all other applications by using a diachylum ointment made according to the following formula: ℞. Ungt. diachyli simplic; olei olivæ, aa. æq. part. Dr. Delaporte has also employed with success the following lotion in pruriginous affections: ℞. Water, 300 grams; glycerin, 100 grams; eau de Cologne, 75 grams; carbonate of soda, 2 grams. These lotions should be repeated whenever the itching is severe, and especially at night, before going to bed. The application should be cold, and should be made with a fine sponge dipped in the lotion. (Practitioner, Jan.)

Pitting in Small-Pox.—I have found the carbolic acid glycerine of the British Pharmacopœia, diluted with four times its weight of glycerine, a most useful application in small-pox. It should be applied as soon as the pustules begin to fill, and be continued until they desquamate. Such treatment, I think, not alone renders the patient less repulsive to those about him or her, but lessens, to a great extent, the pitting resulting from the disease. (Mr. C. W. Thorp, British Medical Journal, Jan. 14.)

Tinea Sycosis.—*Goa Powder.*—Having a case of tinea sycosis, in which a solution of sulphurous acid and also the ung. hydrarg. nit. had failed to cure, I gave the comparatively new remedy, Goa powder, a trial. The result was a speedy cure. I have used it in two cases since with success. Epilation was necessary in one case of long standing where the beard was lifeless. The manner of using it was first to soften and remove the crusts by applying a flax-seed poultice, then using freely the following: Goa powder, gr. xx.; cosmoline, ℥j. M. (Edinburgh Medical Journal, Dec.)

VENEREAL AFFECTIONS.

Can a Man have Syphilis twice?—The man whom we have just seen offers a remarkable example of the occurrence of a second chancre soon after the first. His second sore has been characteristically indurated. He is quite candid, and he makes no doubt that this sore was the result of contagion. Yet it is a barely a year since he had his first chancre, and this was followed by an eruption, of which he had scarcely got clear when this second sore occurred. The case is proof that a man may have an indurated sore on the penis within a year of a former one, but it is not proof that he may have syphilis twice, for this patient has not as yet had any constitutional symptoms as the result of the last chancre. If, however, you ask me for an answer to the general question, Can a man have true complete syphilis twice? then I must reply clearly that he can. Such cases are rare—as rare, perhaps, as examples of second attacks of small-pox—but they do occur. I am at present attending a gentleman who has a terrible phagedenic chancre and rupial eruption, and who unquestionably had complete syphilis, chancre, sore-throat, and rash, seven years ago. I have also a second case under care, very much milder, but illustrating exactly the same fact, with almost precisely similar dates. Second chancres are, however, far more common than second attacks of constitutional syphilis. Many of them are the result of fresh contagion, but seem to have no power to produce constitutional symptoms; but others are not from contagion at all, but form in connection with a taint still remaining from the first attack. It is a most important fact that indurations may form in the penis in every respect exactly like Hunterian chancres, not distinguishable in any way, and yet that they may be merely recurred sores, and the produce of constitutional taint. I have seen this over and over again. In the case of our patient, it is obviously impossible to say, whether or not his present sore is the result of fresh contagion. It may be simply a relapse, or it may be a gumma. He, however, confesses to exposure; and, as the sore followed in due course, it is probably true that he was afresh inoculated. Second attacks of syphilis are sometimes, as in the case just mentioned, very severe. The same has, I believe, been occasionally noted in recurred attacks of variola. As a rule, however, they are mild, or even abortive. Third attacks may even occur; and so may, as we are told, third attacks of small-pox. We must explain such facts, I expect, by reference to individual peculiarity and idiosyncracy, but it is important that they should be known. The belief that syphilis can occur but once in a lifetime is very widely spread among a certain class of the public. I have watched with amusement the change in expression in many a young gentleman's face when he got my reply to his smiling suggestion—"A man cannot, I suppose, have the disease a second time?" (Mr. J. Hutchinson, Brit. Med. Jour., Jan. 7.)

GONORRHŒA.—*New Method of Urethral Irrigation.*—Irrigation judiciously employed in suitable cases and at a proper stage of the complaint, is unquestionably a valuable adjunct to the treatment of gonorrhœa. In the first place, the prolonged stream of water effectually removes the inflammatory products of the disease, which obstinately and intimately adhere to the surface of the urethra, and protect the mucous membrane from the immediate influence of the remedies we endeavour to bring into action, either through the urine medicated by the administration of balsamic drugs, or by the more direct influence of antiblennorrhagic injections. The tenacity with which the secretions adhere has, no doubt, much to do with the difficulties encountered in the treatment of gonorrhœa. It must be remembered that the urethra in health is an elastic tube tolerant of reasonable dilatation; but when invaded by inflammatory changes, it is converted into a comparatively unyielding tube, intolerant of any force having a tendency to expand its calibre; consequently, irrigation, to be safe and successful, must be conducted on a principle consistent with the laws of hydrostatic pressure; and as we propose to use irrigation in a disease which involves conditions opposed to distension, it is required that the mode of irrigation, whilst insuring a continuous and copious stream of fluid, should be free from any undue tendency to stretch the walls of the urethra. At page 234 a new Irrigator fulfilling these conditions is described, with the mode of procedure. (Mr. Walter Whitehead, p. 233.)

SOFT CHANCRES.—*Iodoform.*—The mere dusting of the powder over the sore is almost sufficient to insure a healthy action. Its employment has certainly considerably reduced the duration of this disease, and has done away with the necessity of such painful applications as fuming nitric acid to the exquisitely tender surface. In the out-patient practice of a hospital the use of iodoform will soon banish that most offensive class of cases, the stinking ulcers of the leg. We have long been in the habit of using an ointment composed of iodoform, eucalyptus oil, and vaseline, which has the advantage of enabling the patients to keep their ulcers aseptic whilst changing the dressing themselves daily. It must be owned, however, that this ointment has occasionally set up a rather severe form of dermatitis, due possibly to the fact that iodoform, when dissolved in an essential oil, is apt to undergo decomposition into products of a very acrid nature. Another excellent method of treating ulcers of the leg is to dust the powdered iodoform over them, and then to apply over the sore a piece of the oiled silk protective, and over this a mass of the iodoformised cotton. A firmly applied bandage securing this combines the advantage of a uniform and continuous elastic pressure with that of asepticity. If an ointment such as that described above be employed, and if the patient be directed to use a 5 per cent. carbolic lotion when changing the dressing, it will be found that many smaller abscesses will also remain quite aseptic, though the dressings be frequently changed between the times at which the patient is seen by the surgeon; but if this is to be attempted, it is advisable to incise freely, and thus dispense with the necessity of the drainage tube. (Ed. Medical Times and Gazette, Jan. 14.)

Iodoform appears to be one of the most efficacious drugs in the treatment of the syphilitic non-infecting soft sore, when not unduly inflamed. It has, however, the unfortunate counterbalancing disadvantage of attaching to the patient the liability of unenviable suspicion, and the significant expressions of his companions—the public having become keenly alive to its distinctive and penetrating odour, and having also acquired an appreciative knowledge of the principal purpose for which the drug is most frequently used. The various attempts which have been made to disguise or modify this peculiar odour appear hitherto to have failed. Tonquin beans, kept in the same bottle with the iodoform, succeed to a certain extent and for a limited time. The beans, however, possess a smell which is regarded by many with equal objection to that of iodoform. I have succeeded, I consider, in obviating this objectionable feature, without apparently sacrificing any of the therapeutic advantages of the drug, by using it after the following manner. I first very carefully cleanse and dry the sores by means of little pledgets of bibulous paper, and then, by means of a camel-hair pencil, apply freely over the surface of the sores a solution of iodoform in ether. The ether rapidly evaporates, and leaves the iodoform uniformly spread in an impalpable powder over the sores. To ensure a free application, the latter part of the process may be repeated and allowed to dry. When perfectly dry, each sore is given a coating of collodion, which is allowed to overlap about a quarter of an inch the area of each sore. Before the collodion has had time to dry, a pinch of absorbent cotton-wool is placed on each patch as a protection against the rough contact of clothing. This dressing is allowed to remain undisturbed for twenty-four hours, when the firm film which forms may be gently removed and a fresh coating applied. This treatment is continued day by day until all the sores have quite healed. I have found, on the same principle, and with equal success, that a piece of goldbeaters' skin may be substituted for collodion after the application of the iodoform. By this means, the patient and his associates are spared the disagreeable consciousness of the remedy being employed, whilst the full benefit of the drug is being preserved. A further advantage gained is the protection afforded by the collodion against auto-inoculation, and also against the risk of contagion from others coming in contact with the sores.

The iodoform solution I am in the habit of using has been made for me by Standring, Son, and Co. of Manchester, by dissolving one part of iodoform in two of ether; and, in my Lock Hospital practice, where time and economy is of moment, I have the collodion and iodoform mixed together, the formula being iodoform one part, ether two parts, collodion to ten parts. In the latter solution, the odour cannot be said to be wholly disguised, but it is sufficiently concealed for all practical purposes. (Mr. Walter Whitehead, British Medical Journal, March 11.)

SYPHILITIC CHANCRE. — *Non-Specific Treatment.* — Whatever be the amount of induration or ulceration connected with this sore, the treatment of it should be conducted on ordinary surgical principles. No mercury is required either internally or externally, and the sore will be found to behave very much the same as other ulcers do under similar conditions when submitted to rational treatment. Caustics are rarely necessary, and should only be applied when they will assist the process of healing, and not under the expectation that even in the earliest stage of the chancre they may ward off constitutional affection. I hold with Ricord that the appearance of the chancre is the first indication that the blood is contaminated by the syphilitic virus. When it is considered necessary to apply nitric acid for surgical reasons, a plan has been suggested by Dr. Keyes of placing first a drop of pure carbolic acid on the sore, and then drying the surface with blotting paper, before applying the stronger acid, the well-known anæsthetic properties of carbolic acid making the subsequent application less painful. There is an attempt at present to resuscitate the treatment by excision of the chancre, in order to prevent constitutional infection. This practice has not been found successful, for reasons stated above when speaking of cauterisation. Water dressing or any of the ordinary astringent lotions may be applied according to indications. Boracic ointment and iodoform are also useful applications, more especially the latter. It is best to apply iodoform in powder, and if it were not for its persistent smell it would be very generally applicable. The addition of tannin has been recommended to lessen the smell of iodoform, but it makes little difference. By these simple means the worst forms of chancre will heal, although it may be necessary also to give tonics to improve the condition of the patient, when that is required. Among some of the first symptoms of constitutional syphilis, appearing often before any skin eruption, are periosteal swellings and bone pains of a more or less transient character, the pains being worst at night, and having a tendency to subside on the development of the skin eruption. For these symptoms iodide of potass in 10 grain doses is useful, but only as directed against the bone and periosteal symptoms, and not as having any special reference to this the so-called secondary stage of syphilis. I believe iodide of potass to be practically useless for the relief of any symptoms occurring during the secondary stage, except those above referred to. (Mr. F. Cadell, p. 262.)

AFFECTIONS OF THE EYE AND EAR.

Acute Purulent Catarrh of the Middle Ear.—When the tympanum is full of pus, wash it out as Mr. Langton describes. He says, "Seeing the perforations had only existed for twenty-four hours at the utmost, I determined to try the value of the forcible injection of a weak alkaline solution through the tympanum, so as to cleanse out, as thoroughly as possible, the tympanic cavity from all purulent secretion. With this view, I filled a brass syringe with a warm solution of soda (consisting of five grains of domestic soda to an ounce of water). Over the nozzle of the syringe was drawn tightly a piece of india-rubber tubing, so that it might fit accurately the external auditory meatus. The contents were then very cautiously and slowly injected through the tympana and the Eustachian tubes, and escaped through the corresponding nostril. About half an ounce of this fluid was injected through each tympanum. The patient afterwards expressed himself as feeling relieved of much of the previous throbbing and sense of fulness in the ears, although the acoustic power was not appreciably increased. The next day all the unfavourable conditions had improved, there being less pain, throbbing, and discharge. The perforations seemed hardly so large, and the membranes were less vascular. A watch could be heard at a distance of half an inch on each side. Both tympana were again injected after the same manner as yesterday, but with a very diluted solution. Day by day, the discharge diminished and became less purulent; with its diminution the hearing power improved, so that in a few days after the first injection the patient's hearing increased to six inches on the left, and to four inches on the right side. Within eight days the perforations had completely healed, attended with almost normal hearing on both sides. I saw this patient about a year afterwards, and ascertained that he had had no further aural trouble, and that his hearing was as good as ever. (Mr. John Langton, St. Bartholomew's Hospital Reports, 1881.)

OTORRHŒA, OZÆNA, ULCERS OF THE SEPTUM NASI, ETC.—*Iodoform.*—Iodoform is extremely useful in cases of otorrhœa, ozæna, ulcers of the septum nasi, &c. In the treatment of these diseases it may be applied either alone or in combination with any other powder, the employment of which the particular case may render advisable—bismuth, tannic acid, oxide of zinc, or what not. It is easy to blow the powder up the particular part in question, and we would suggest that by means of a speculum it might be used in a similar way in the treatment of vaginitis, though we do not profess to speak on this subject from experience; it may be suggested, however, that a plug of iodoform cotton, inserted into the vagina, might enable the surgeon to perform a strictly aseptic abdominal section in a case where it was impossible to avoid interfering with the vagina or the uterus. (Ed. Medical Times and Gazette, Jan. 14.)

PSEUDO-MEMBRANOUS DIPHTHERITIC CONJUNCTIVITIS.—As soon as the nature of the disease was definitely recognised, all other treatment, if any, was stopped, and quinine lotion, containing four grains of sulphate of quinine, with a minimum of dilute sulphuric acid to an ounce of water, was alone employed. As far as possible the diseased surfaces were kept constantly bathed with the solution, the conjunctival sac being converted, as it were, into a trough holding the quinine lotion. A bowl of the solution was also placed within reach of the patient, who washed the eye frequently and kept a well-soaked compress constantly applied in the intervals. Besides these applications by the patient and by the nurse, the house-surgeon visited each case three or four times a day. On these occasions the lids were everted and the conjunctival sac thoroughly cleansed with the quinine lotion. The superficial disintegrated portions of the exudations were then gently removed with wet lint, care being taken not to aggravate the inflammation by rough handling or by rude attempts to tear off the pseudo-membrane. Usually, the quinine lotion was iced. In two cases the local application of powdered sulphate of quinine was tried at first, or sulphate of quinine rubbed up with an equal part of calomel; but, in addition to causing great pain, the powder did not appear to be so beneficial as the quinine in solution, and was therefore soon abandoned. The virtues of the quinine are, I believe, specific in the diphtheritic exudation, and manifest themselves by checking or controlling the inflammatory process and the limiting the inflammatory neoplasms. Whatever may be the influence of the minute organisms that are said by some writers to be essential factors in producing diphtheria, it is certain that in diphtheritic ophthalmia the cornea is destroyed by the degree of the inflammation and the amount of concurrent exudation. The bacteria or vibrios or micrococci (for there is a hopeless disagreement as to which are the noxious organisms) do not destroy the cornea; nor, on the other hand, do strong applications of carbolic acid, which is, we are told, so fatal to "germs," arrest the progress of the pernicious pseudo-membrane in time to save the cornea. Three of the cases to which I have referred were at first treated by frequent ablutions with a 5 per cent. solution of carbolic acid, and in every instance the pseudo-membranes rapidly spread under these applications, whereas they were immediately controlled by the quinine lotion. (Prof. John Tweedy, p. 257.)

STRUMOUS OPHTHALMIA.—*Sulphide of Calcium.*—The sulphide of calcium will be found particularly serviceable in those cases of children with manifest strumous habit, enlarged cervical glands, swollen face, the eyelids tightly closed, photophobia, and where, on opening the eyes, a gush of hot tears is emitted, and examination of the ocular surface discloses one or more phlyctenules on the cornea, or it may be merely increased vascularity of conjunctiva. These cases treated by the ordinary constitutional and local remedies are often tedious, but with the sulphide of calcium, coupled with the usual applications to the eyes, such as atropine and warm fomentations of poppy, or what not, frequently quickly yield a happy result. In other cases also of phlyctenular conjunctivitis or keratitis, and not alone in children, the good effects of this medicine are conspicuous. Of course, like all other drugs, it will be hardly likely to be suitable for, or to benefit, all cases, but I have now employed it with good results so frequently that I am quite satisfied as to its being a useful remedy. After little or no benefit with steel in its various forms, and cod-liver oil, the rapid recovery often after the substitution of the sulphide has been astonishing. The mode of administration is generally in the form of a powder, and from gr. $\frac{1}{10}$ to gr. $\frac{1}{4}$ of the sulphide, with a few grains of sugar of milk, are given about three times daily. In this way children take it readily. (Mr. S. Snell, p. 260.)

MIDWIFERY, ETC.

ANTISEPTIC OVARIOTOMY.—*Recent Improvements.*—As the material for tying vessels and uniting the wound, the same pure twisted silk, unmixed with any vegetable fibre, which I have trusted to for about twenty years, has been used. I have never tried catgut; and after trial, have abandoned whipcord, hempen ligatures, silver iron and platinum wire, horsehair and other materials. Various forms of quilled and twisted sutures have also been tried and abandoned. But since 1878, all the silk for ligatures and sutures has been soaked before use in a five per cent. solution of carbolic acid or phenol. Dry dressing of the wound has been continued; but in place of the pads formerly used, of 5 per cent. of oil of tar with 95 per cent. of chalk, either thymol gauze, or cotton pads charged with borax or phenol have been used. These are more comfortable to the patient, and are better absorbents of moisture. As a rule, they are not touched before the seventh or eighth day, when the sutures are removed, and the wound is almost invariably found to be completely united. The two most important additions to previous antiseptic precautions are, first, carbolising the sponges and instruments, and secondly, the use of the spray. I have long insisted on the great importance of always using perfectly pure sponges, and I believe this object is more perfectly attained by soaking them in carbolised solution after washing, than by washing alone. After an operation, I continue my old plan of keeping the cleansed sponges in a weak solution of sulphurous acid. And during the operation, in addition to washing in pure water, every sponge before use is wetted with a 2 to 3 per cent. solution of carbolic acid or absolute phenol. Soft clean linen cloths, wetted with a warm solution of phenol, may be used to lessen the number of sponges required; and nurses must be cautioned not to put any of the soiled sponges into the solution after until they have been washed, otherwise albumen may be so coagulated as to prevent thorough cleansing. As nurses often fall into this error, it is well to have two or three different sets of sponges, all carefully numbered, kept separate for the successive steps of the operation. Nearly all the instruments used in ovariotomy may be protected from rust by a coating of nickel. They are then more easily and thoroughly cleaned after use, and the cleaned instruments should be placed before, and replaced during, the operation in trays or dishes filled with a warm solution of phenol. These additional precautions as to sponges, silk, and instruments, I believe to be really important. I feel still doubtful about the spray. In prolonged operations, I have had reason to fear that its chilling effect upon the patients has been injurious. But I have never once seen any other ill effect which I could attribute to it, nor anything like carbolic poisoning. The misty cloud occasionally obscures the field of operation, but not to any serious extent, and it is always easy to protect the peritoneal cavity by a large warm sponge against the continued action of the spray. After a few trials I gave up thymol spray as useless, and for more than a year past have used a spray of absolute phenol of a strength of one in forty. And this I continue to use, believing it to be safer than the irrigation or sponging proposed as substitutes, although I fully admit that we require a far greater number of trustworthy experiments, or of comparative observations made under similar conditions with and without spray than have yet been made known, before we can receive any satisfactory answer to the questions whether carbolised vapour or air can destroy or render innocuous, infective or putrefactive substances or germs floating in the air; or what is the share which the spray, among other additional antiseptic precautions, has had in obtaining the better results which have undoubtedly accompanied their combined employment. (Mr. T. Spencer Wells, p. 306.)

Hyperpyrexia after Listerian Ovariotomy.—So far as my own practice is concerned the fact is this: that as regards temperature the Listerian method, in a series of thirty-six cases, shows in its favour only a difference of four-tenths of a degree, taking the average of the first four days, over another series of the same number done previously. I have proved that the claim that has been set up for Listerism cannot be sustained (in preventing pyrexia after operations); that carbolic acid may be introduced into the circulation in poisonous quantity by means of this method; that when so introduced it manifests its presence by producing a state of hyperpyrexia, and that thus it actually produces too often what it was intended to prevent. And the practical result of all this, so far as I am personally concerned, is that, to meet the evil which to me is so formidable, I have gradually diluted my spray and solutions so as to reduce the whole question to one of *cleanliness*, which, after all, is the true secret and merit of Listerism, and I am happy to say with the effect of very greatly adding to the success of my ovariotomy work. (Dr. Granville Bantock, p. 299.)

DISPLACEMENTS OF THE UTERUS.—*New Operation for Inveterate Cases.*—The uterus, when in the normal position, is scarcely subjected to any force that tends to produce protrusion. The sacrum and coccyx bear the greater part of the pressure in one direction, and the pubic bones bear the pressure in another direction. Whatever maintains the uterus in this normal position is the great preventive of prolapse. For the virgin uterus, the natural resilience and coaptation of the surrounding organs is probably sufficient; but when these fail, the *round ligaments* are the chief agents. These ligaments are "attached to the upper angles of the uterus, one on either side, immediately in front of the Fallopian tube. From this point each ligament proceeds upwards, outwards, and forwards, to gain the internal inguinal ring; and after having passed, like the spermatic cord in the male, through the inguinal canal, reaches the fore part of the pubic symphysis, where its fibres expand and become united with the substance of the mons veneris. Besides areolar tissue and vessels, the round ligaments contain plain muscular fibres, like those of the uterus, from which, indeed, they are prolonged." (Quain.) These ligaments, so placed as to manifestly prevent backward or forward swaying of the fundus uteri, are unfortunately subject to stretching, as nearly all soft tissues are, provided the force be persistent enough. This force arises in repeated pregnancies, uterine congestions, and from other causes well known to gynæcologists, that change the position of the uterine wedge, and overcome the *staying* force of the round ligaments by stretching them. If complete retroflexion or retroversion takes place, prolapse is not so serious; but it is when the retroversion is partial, when the uterus comes more into a line with the axis of the pelvic outlet, that the pressure of the adominal contents acts with most efficiency, and with a persistence that is certain to produce protrusion, if not complete prolapse. If we could replace the uterus, and maintain it when replaced in its normal position and direction, we should at once take away its tendency to prolapse. We cannot easily increase the resiliency of the surrounding tissues, but we can do what my operation consists in, and that is "*pull out the slack of the round ligaments.*" The operation is performed by cutting down upon each abdominal ring, gathering up the ends of the ligaments, freeing each from its nerve, and gradually releasing them by patient and cautious traction from the neighbouring tissues, until the position of the uterus, as ascertained by the finger in the vagina, satisfies the operator. The ligament is then stitched to the tissues around the ring, and the loose ends attached to each other or rolled round two pieces of wood, which are fastened together in the middle line. The picking up of the ends of the ligament is the *difficult* point, and the freeing of the ligaments from their surroundings is the *delicate* point, but by experience both can be performed easily and effectually. The ligament slides within its sheath, and the peritoneum is not disturbed. No risk of hernia or of pelvic inflammation occurs. Beyond some pain for the first few days, the operation is harmless if carefully performed, but experiments on the dead subject have shown me that danger may arise from incautious operators. (Dr. W. Alexander, p. 280.)

DYSMENORRHŒA.—*Spasmodic.*—Dilating the cervix uteri for spasmodic dysmenorrhœa properly conducted is not a dangerous one—is, indeed, safer than any other. The first bougie passed should be a small one, and there should not be sufficient difference between the size of it and that which follows to cause a splitting of the cervical canal, an occurrence which is likely to arise under other circumstances; the dilatation should, therefore, be performed at one time, and not on successive days, as hitherto recommended. I do not mean to assert that dilatation by bougies is always successful; if it fail, other modes of treatment may be tried, the insertion of a silver stem, for instance, or division of the cervix. But I look upon these as more complicated and attended by more risk. I have seen so much harm follow the careless introduction of stem pessaries that I never venture to insert one without keeping the patient at absolute rest for two or three days afterwards, whereas the process of dilatation can be conducted with safety in one's own consulting room, the patient walking away and returning home afterwards. And, with regard to incisions, I was in the habit of just nicking the sides of the external os with the puncturing lancet in the out-patient room, until several cases of inflammatory mischief causing fixation of the uterus followed, and I was compelled to discontinue the practice. My cases show that a very small amount of dilatation is necessary; in three out of the five cases two bougies only, Nos. 7 and 8, having been passed—and an important point from my observations, upon which I would lay stress, is that the time selected for performing the dilatation should be within a week or ten days after a monthly period, and not close upon an approaching one. The points I would place before the Society are: 1st. *How do the dilators act in relieving the dysmenorrhœa?* I have already suggested that the pain seems to be due to spasm, which at the approach of the menstrual flux seizes upon the uterus, the endometrium of which is in a state of hyperæsthesia. The contact of a foreign body with this seems at first to increase its morbid sensibility, but as the structure gets accustomed to its presence its sensibility becomes blunted, and the spasm subsides. The impression left upon the endometrium after the withdrawal of the bougies is such as to render it less sensitive and liable to spasm. How is this explained? 2ndly. *The dysmenorrhœa being got rid of, pregnancy occurs.* In none of my cases was there either stenosis or constriction of the canal by acute flexion. The theory, therefore, of permanent constriction being discarded, in what did the impediment lay? Was it a spasmodic constriction causing ejectment of the semen? It is possible that in Case 2 it might have been due to a large plug of coagulated mucus, for such was observed to come away during the dilatation, and pregnancy occurred immediately afterwards. But in none of the other cases was this noticed, and in three of them pregnancy did not ensue for a period of from two to four months. The idea of obstruction from a plug of mucus, therefore, must be rejected. (Dr. Clement Godson, p. 294.)

Anteflexion of Womb and Dysmenorrhœa.—The general conclusions which I draw from the effect of treatment are—First, That dysmenorrhœa associated with anteflexion is frequently cured without straightening the uterus. Second, That straightening the uterus does not invariably cure the dysmenorrrhœa, and that there is no evidence that it does so frequently. I submit the following propositions:—1. That there is no anatomical evidence that anteflexion causes any appreciable hindrance to the escape of menstrual fluid. 2. That there is reason to think that well-marked anteflexion is present in nearly half of all women who have not borne children. 3. That therefore it is to be expected that anteflexion and dysmenorrhœa would frequently coincide. 4. That dysmenorrhœa is practically as common when the uterus is straight as when it is anteflexed. 5. That painless menstruation is practically as common when the uterus is anteflexed as when it is not. 6. That when dysmenorrhœa and flexion go together, the severity of the pain bears no relation to the degree of the bending. 7. That dysmenorrhœa associated with anteflexion is frequently cured without straightening the uterus. 8. That there is no evidence that straightening the uterus invariably, or even frequently, removes dysmenorrhœa which is associated with anteflexion, and in which other methods of cure have been ineffectual. 9. That these facts tend to show that the relation between anteflexion and painful menstruation is not that of cause and effect, but merely that of coincidence. (Dr. G. Ernest Herman, p. 288.)

HEGAR'S UTERINE DILATORS.—The idea of dilating the cervix by graduated bougies is not original with Hegar, but was first proposed by Peaslee, and both Hanks and Lawson Tait have introduced graduated uterine dilators made of vulcanite. Hegar's dilators differ, however, very essentially from these, both as to their shape and also as to the number of dilators that are considered requisite. Hanks' dilators consist of a set of ten hollow vulcanite ovoids, which can be screwed into a common handle. Mr. Tait's dilators are only four in number, are conical in shape, and also fit into a common handle. Hegar's consist of twenty-five pieces, each piece being about 5 inches long and cylindrical in shape. At one end the cylinder is flattened out for a distance of $1\frac{1}{4}$ inches, so as to form a sort of handle. The first piece has a diameter of 2 millimetres, and the diameter of each successive piece increases by half this amount, or 1mm. In order to introduce them the woman is placed on her side in Sims' position and his speculum is introduced. The cervix is then fixed with a vulsellum, and then one of the dilators is introduced till it passes the internal os. If it goes in quite readily. it is at once removed and the next larger size introduced. If, on the contrary, there was much difficulty experienced, the dilator is left for from 5 to 10 minutes *in situ*, at the end of which time the next number can be easily introduced. As a rule, we may commence with No. 5-7 in women who have borne children, and with No. 3-4 in those who have not. Each dilator is stamped with the same number as there are millimetres in its diameter. It is a curious fact that the resistance to dilatation sometimes becomes much less after a certain amount of dilatation has been effected. (Dr. Macan, Dr. Atthill, p. 285.)

HOUR-GLASS CONTRACTION OF UTERUS.—*Nitrite of Amyl.*—I found it impossible to get my hand into the uterus to deliver the placenta. Bearing in mind the remarkable power which nitrite of amyl possesses in relaxing tension in the blood-vessels, I determined to test its action on the uterine spasm. The patient had three drops of the nitrite of amyl given her on a handkerchief to inhale. During the inhalation, the ring of muscular fibres round the os internum, which had been so rigid as to be absolutely undilatable, steadily yielded, until I could pass the whole hand into the uterus and detach the placenta, which was universally adherent. We possess in ergot a great, a dangerous power of augmenting the force of the uterus. We want an agent endowed with the opposite effect, that will control and suppress uterine action. I consulted Dr. Richardson on this point. He tells me the desired power exists in the nitrite of amyl. Three minims of this added to one drachm of ether taken by inhalation is the form he recommends. It does not produce unconsciousness; but it is an anæsthetic as well as a sedative of muscular action. It is the antidote or opposite force to ergot. In it we have the desiderated 'epechontocic agent.' I am not aware that nitrite of amyl has been used to relax uterine spasm before. In it we possess, I think, a new and trustworthy addition to the resources at command for overcoming spasmodic or trismic contractions, which will not always yield to other remedies. (Dr. Fancourt Barnes, p. 268.)

MIDWIFERY FORCEPS.—The advantages of Dr. Barnes's instrument are these: 1. Lightness of make; 2. Narrowness of blades; 3. Straightness of blades, the pelvic curve not being nearly as well marked as in Simpson's; 4. Their length. Consequently, from all these points we have an instrument capable of doing almost anything that the forceps should be expected to do. Their suitability for bad occipito-posterior cases where rotation is necessary will be at once apparent, and I have found their benefit in such cases. Simpson's has the finger-rests, but I doubt if the loss of them makes much difference, as you can get your fingers inside the lock in Barnes's. Whatever may be the true merits of each, the case related is proof positive of the advantage of Barnes's instrument in a certain class of labours. (Mr. Erskine Stuart, Edinburgh Medical Journal, Dec.)

Axis-Traction Forceps.—Taking Simpson's "intermediate" forceps to work upon, I have added about two inches to the length of the stem of the blades, that is, of the part intervening between the commencement of the cranial curve and the lock. This additional length I have so bent that, with the blade *in situ*, it shall curve round the fourchette and pass backwards over the perinæum until it touches a line prolonged downwards through the traction axis of the blades. At this point I have again bent the stem, so that the remaining part of the added length and the handle thereon shall continue in the traction axis line. Such is the modification. Briefly, I have neutralised the pelvic curve by the addition of a perineal curve, so that the focus of resistance lies in the line of traction; and the instrument, in this respect, becomes practically a straight forceps. Nor is this advantage obtained at any cost, except the addition of one and a half inches to the total length of the instrument. The "perineal curve" simply re-confers the power of axis traction which was sacrificed by the adoption of the pelvic curve, and without limiting the range of applicability bestowed by the latter. Therefore in the ordinary "intermediate" forceps, thus modified, we have an instrument of the widest utility, endowed with the power of axis traction; suitable wherever forceps are required, except where the obstruction is exceedingly high; and neither unhandy in the simplest nor inefficient in the most difficult cases. This modification is also valuable since it is equally applicable to the "long" forceps, and, moreover, it deprives the short straight forceps of the single superiority it possesses. (Dr. S. Macvie, p. 270.)

PAGET'S DISEASE OF THE NIPPLE.—The general conclusion to which Mr. Butlin, F.R.C.S. (who contributed two papers to the Royal Med. and Chir. Society in 1876-7) came was, that cancer of the breast is essentially a disease of the epithelium of the mammary gland. In the discussion that ensued on the reading of Mr. Butlin's paper, Sir James Paget is represented as having said that "he could only speak of the clinical characters of these cases. He had no doubt of the frequency with which chronic eczema or any chronic irritative disease of the breast or nipple was followed by cancer. He had now seen some fifteen or twenty such cases, and regarded the sequence as so certain that he would venture to prophesy that if any irritative condition were present in the skin of the areola and nipple for a period of three or four years (the patient being of an age favourable to cancer), cancer of the breast would ensue. The cases described by Mr. Butlin were examples of 'eczema;' but he had met with two or three instances which might more fitly be termed 'psoriasis;' others in which only an excoriated surface was present; but all had the same issue—viz., the development of cancer, not in the skin, but in the substance of the gland." Mr. Butlin's views regarding the nature of cancer of this special organ, afford an easy transition to the more general proposition which many histologists hold—viz., that all varieties of cancer have an epithelial origin. Everybody knows that prolonged local treatment of a malignant sore not only becomes ineffectual in the end, but increases its activity, and lessens the chance of successfully eradicating it by early removal. The early diagnosis of "Paget's Disease" is, therefore, of paramount importance. On this point, Dr. George Thin writes as follows:—"In distinguishing, clinically, this malignant dermatitis from eczema, the chief points to be borne in mind are the well defined margin in the former, and the evidence, when the tissue is grasped between the fingers, of infiltration in the papillary layer. An eczema of the areola, that had the thickening and moist redness which have characterised some of the cases of this affection, would be so acute in its nature that the zone of inflammation would pass off gradually into the surrounding skin, would probably become widespread, and would give rise to very active symptoms. The overlapping margin forming a veritable ridge, with a sulcus behind it, which characterised another case, would of itself be sufficient to exclude the diagnosis of eczema. The history of the case may be found useful in establishing the diagnosis. In two cases which have come under my notice, there was clear evidence that the disease began in a position corresponding to the openings of the lactiferous ducts, and remained limited in that position for some time before extending over the surface of the nipple." (Dr. R. Munro, p. 315.)

PESSARIES.—Dr. Meadows showed (at a meeting of the Obstetrical Society) pessaries made of the new material zylonite, a compound of paper and nitro-glycerine. It was more elastic than vulcanite, and could be more completely softened for moulding by boiling water. (Dr. A. Meadows, Obstetrical Trans., 1881.)

Spring Pessaries.—Dr. Beverley Cole, of San Francisco, showed an instrument designed to meet the indications of a large class of cases of retroversion and flexion, in which from exquisite sensibility, whether primary or secondary, the employment of ordinary pessaries is contraindicated or inadmissible. It consists of a common Hodge or Smith's pessary, the upper arm of which is shorter than the original instrument, and at the same time less curved, so as merely to adapt it to the curve of the floor of the vagina. To either lateral arm a segment of a watch spring, about one and a half inch in length, is attached, these two supporting at their distal extremity a bar of vulcanite or celluloid—this latter being intended to occupy the fornix of the vagina and uterus posteriorly. The advantage of the pessary so constructed is that the uterus instead of resting on a *fixed bar* is allowed to ride, so to speak, upon the spring, obviating the inconvenience of the older arrangement. A woman wearing this instrument finds great comfort. Whilst the womb is raised, or rather supported, when properly replaced, unlike other pessaries, in the act of defecation the bar of the instrument is pushed forward by the accumulated excrementitial material above, and the latter thus escapes without trouble, whereas, in the ordinary Hodge and other pessaries the *bar* is caught by the descending mass and depressed, and carried backwards so as to increase the difficulty attending this act. He also showed a "Spring Anteversion Pessary." Like the other, it is a common Hodge or Smith instrument to serve as a bed, and a spring attached to either lateral arm or bar. But in this these springs run in the opposite direction, in other words, run forwards. They, however, support a bar which is designed to exert pressure upon the fundus of the organ (like all others) through the anterior wall of the vagina and the bladder. All who have had experience in the treatment of anteversion or flexions well know how sensitive the uterus is, and frequently irritability of the bladder is encountered, and how difficult it is to secure proper support through instruments because of this sensitiveness. This pessary supports the womb and allows it to ride upon its upper bar, enabling the woman to engage in all usual occupations, even to dancing, without complaining of the pressure, or experiencing any discomfort. (Dr. B. Cole, Obstetrical Transactions, 1881.)

RETROVERSION OF THE GRAVID UTERUS.—*Fluid Pressure.*—The efficacy of fluid pressure applied to the reduction of inverted uterus was first practically demonstrated by Dr. Tyler Smith in 1868, and since then has been successfully adopted by others. The same principle may with equal benefit be applied to the reduction of some cases of retroversion of the gravid uterus. This I first carried out at the London Hospital; and it has been recently repeated by Mr. Hunt, obstetric assistant at Guy's Hospital. A woman aged 23, pregnant three months and a half, was admitted under my care for retention of urine. Sixty-four ounces were drawn off. The uterus was acutely retroflexed, the body being locked under the promontory. Reduction by taxis appearing to demand an undue amount of force, one of "Barnes's bags" was passed into the rectum, and distended with water. In one hour, it was found that the uterus was completely restored to its position. She went her full time, and was safely delivered. In this case, perseverance in the taxis would probably have entailed so much uneven pressure on the uterus as to have led to abortion and metritis. The sustained, equable, diffused water-pressure accomplished the reduction gradually, smoothly, and safely. "Plus fait douceur que violence." (Dr. Robert Barnes, British Medical Journal, Dec. 10.)

SUBINVOLUTION OF THE UTERUS.—When we speak of subinvolution of the uterus, we mean that the process by which the womb regains its original size subsequent to delivery, or abortion, has been from some cause retarded or arrested; this process has been termed involution, and when it is incomplete we talk of the uterus as being in a condition of imperfect involution, or more commonly, of subinvolution. Various agents are used for the purpose of stimulating the uterus to contract and of lessening the irritability of the intra-uterine mucous membrane, all of these must, to produce any satisfactory effect, be applied to the interior of the uterus, and, indeed, be carried up to the fundus; those most commonly employed are—(1) carbolic acid; (2) tincture of iodine; (3) iodine dissolved in carbolic acid (a preparation to which the name of iodised phenol has been given); (4) nitric acid; and (5) the solid nitrate of silver. I give them in the order in which I recommend their employment. In recent cases of subinvolution I seldom employ any agent except carbolic acid, it is mild in its effects, seldom causes much pain, and, with care, is quite safe. Let me here give you one rule from which there should be no exception—namely, never apply carbolic acid or any other agent to the cavity of the uterus unless the os internum is sufficiently patulous to permit of the easy passage of the probe through it. If you neglect the observance of this rule you will certainly get into trouble. Carbolic acid, tincture of iodine, or the iodised phenol should be applied about every third or fourth day, and if much congestion be present the cervix can be punctured immediately upon the application being made. This will have the double effect of relieving pain should the carbolic acid have caused any, and by lessening the congestion facilitate the contraction of the muscular fibres of the uterus. Carbolic acid applied to the surface of the uterus makes a superficial slough; in fact, it blisters gently the intra-uterine surface. At the moment of application some pain is usually felt, but this rapidly dies away, and, indeed, it often relieves previously existing pain, especially when that is due to hyperæsthesia of the uterus. Nitric acid or the nitrate of silver are not suitable in recent cases. (Dr. Lombe Atthill, p. 273.)

UTERINE HEMORRHAGE. — Let me call attention to uterine hemorrhage, either post-partum, or in the common form of menorrhagia in young girls or adults of more advanced years. Of post-partum hemorrhage one typical case will suffice, though others might be given. Mrs. K——, aged twenty-eight. Fourth pregnancy. (My first attendance on her.) Always has most severe flooding after delivery. Nov. 1875, delivered of twins; within five minutes a pailful of blood (two to three gallons). Nov. 1876 (three weeks short of twelve months), again delivered of twins with similar hemorrhage. (All four children still living.) March 1878, delivered of a living boy. Lombe Atthill's warm-water injection soon stopped flooding, which was again most severe. Dec. 1880, again delivered of twins (one only alive.) When summoned to her this time, one-third of a grain of ergotine and one-sixtieth of a grain of sulphate of strychnine, given every quarter of an hour, had the effect of strengthening the uterine contractions, completing the labour expeditiously, and relieving the attendants of the previous dreaded and frightful concomitant—the hemorrhage,—for the placentæ were expelled without the slightest bleeding, and the patient was about in little over a week, the last visit being paid on the sixth day. An equally good result was obtained, a month ago, in a primapara with atony of the uterus, delivery being effected by the forceps, when hemorrhage was prevented by similar means. In the treatment of menorrhagia the same remedies are given in like quantities, but three or four times a day, and during the intervals also, together with the arseniate of iron in $\frac{1}{30}$ of a grain doses. A young lady, aged sixteen, who had become quite blanched and exhausted by the great drain of blood, which lasted a week and returned in a week (thus she was losing one-half her time), under the above treatment was restored to health in about five weeks, and another was similarly relieved in seven weeks. A kindred case to the above may here be mentioned—viz., hemoptysis in a man in his fifth decade, who recovered under the employment of ergotine and strychnine, after various kinds of treatment had failed. (Dr. Benj. Walker, Lancet, Dec. 10.)

UTERINE TUMOURS.—*Operations for Removal.*—What we have to fear in these cases, and in those in which the cavity of the uterus is opened in removing fibro-cysts or solid outgrowths, is *sepsis.* What we have to find out is how to render these operations really *aseptic*, or how to deal with the divided uterine wall in such a way as to render the septic material least dangerous. Suture of the peritoneal edges of the uterine wound, so as at once to close the peritoneal cavity, the raw surfaces being left without suture towards the interior of the uterine cavity, in order that discharges may escape by the cervical canal, seems to answer in some cases; but this method has one great disadvantage. I am firmly convinced, from what I have seen of this branch of uterine surgery since the introduction of antiseptics, that we only want a surely aseptic method to render complete and partial removal of the uterus as little fatal as ovariotomy. *Conclusions*:—1. In the simple cases patients recover under Lister's method with a certainty previously unknown. 2. There is less fever, and convalescence is more rapid than under the old method. 3. The success obtained in the more complicated cases is in proportion to the exactness with which the antiseptic method can be applied to the individual cases. 4. The accidents and complications occasionally following operations, such as hemorrhage, for example, are more easily overcome in cases which are strictly aseptic. 5. There are difficulties, and even dangers, in the application of the method, and the more experience the individual surgeon has in it, the more readily he foresees and avoids these, and the more complete becomes his success in applying it. (Mr. J. Knowsley Thornton, p. 311.)

MISCELLANEA.

ANÆSTHESIA.—ETHER v. CHLOROFORM.—It is confessedly difficult, perhaps even impossible, to settle by statistics the question of the relative danger of these two anæsthetics; chiefly for the reason that, whilst we know pretty nearly how many deaths from each agent occur during the year, we have not the means of ascertaining the relative proportions of the cases in which each anæsthetic has been used. Such being the case, it may be worth while to record the opinions of those who, having for a great number of years had experience of chloroform, have also for many years (in my own case, more than six years) almost abandoned it in favour of ether. I wish, therefore, to tender my conclusions for what they are worth, based, as they are, upon what I have seen in the practice of my colleagues and myself at the Leeds Infirmary, and upon my experience of anæsthetics in my private practice. My conclusions are as follows.—1. Ether, *properly administered*, is a much safer anæsthetic than chloroform. So much safer do I believe it to be, that I counsel every surgeon whom I can influence in the matter, to study the method of its right administration, and to let ether take the place of chloroform. The exceptions I make in favour of chloroform are: in infants, in patients subject to asthma or chronic bronchitis, and also, perhaps, in cases of abdominal obstruction, with difficult breathing, in which an operation has to be performed. 2. When many operations have to be done in rapid succession, to use ether is a great economy of time. A good "etherist" can get most patients under its influence in from one and a half to two minutes, whereas, in my experience, chloroform must be given from six to fifteen minutes before an operation can be commenced. I am aware that chloroformists trained in Edinburgh usually administer chloroform more rapidly than those trained in English hospitals. 3. A patient under the influence of ether is far more passive, and therefore in a more convenient condition for operation, than one under chloroform. As soon as the effect of chloroform is passing off, the patient becomes, as a rule, and often very suddenly, very sensitive to pain; whereas, in the case of ether, especially if the patient have been kept for some time under its influence, the return of sensibility to pain is very slow. In fact, a patient may become so far conscious as to converse with the surgeon whilst stitches are being placed in the wound, and at the same time be entirely unconscious of pain. This was not my experience of chloroform. 4. When ether is administered without food on the stomach, troublesome sickness is very rare. 5. In using ether, the *safety and comfort of the patient*, the *rapidity of the anæsthesia*, and the *convenience of the surgeon* in operating, depend very directly upon the *method* of administration employed, and the *manner* in which the operator does his work. 6. There are good methods of administration of ether and bad methods, and there are good and bad "etherists." The varying opinions of the value of ether which prevail in the profession probably depend very directly upon the varying methods and manners of administration. 7. It is a bad method to give ether "on a towel," as first taught us by Dr. Joy Jeffreys, to whom England is deeply indebted for his successful crusade in favour of ether. This involves a great waste of ether, ten to twenty ounces being required. The patient's lungs are chilled, and bronchial *râles*, struggling, and maniacal excitement not unfrequently result. I, along with my colleagues, commenced ether under this system as a duty, and by no means an agreeable one, and held on doubtfully when I suspected that some patients probably died from the effects of the chilling of the lungs. 8. It is a bad method to give ether with the American basket-work frame, which, though not much better than the towel, served a good purpose as a step to better things. 9. The good methods are those in which the patient breathes over ether into an India-rubber bag, a method, I believe, introduced into practice by Dr. Ormsby, of Dublin, and carried to further perfection by Mr. Clover. In this method, the patient breathes the same air over and over again for six or eight times, thereby economising the heat of the air-passages, economising ether, and enhancing the effect of the ether by partial asphyxia. My experience of the use of Clover's smaller inhaler, under good management, is this: *a.* A patient can generally be ready for operation in a minute and a half, sometimes in less than a minute; *b.* There is rarely any struggling; *c.* Noisy excitement hardly ever occurs—perhaps, in my private practice, once in a hundred times; *d. Râles* in the trachea are but seldom heard; *e.* Instead of six or eight ounces of ether being used in a short operation, and sixteen to twenty in one lasting an hour or an hour and a half, half an ounce or less suffices for a short operation (such as "sphincter-stretching" or "iridectomy"), and two to three ounces for an operation of an hour's duration, such as colotomy or excision of a joint. 10. The administration of ether by inferior methods is still too common, and was until recently prevalent in some of our larger hospitals. 11. Even with Ormsby's or Clover's inhalers, there is an infinite variety of skill in different etherists. 12. In order to become a good etherist, the administrator must *study how* to give ether, must *watch* the patient *attentively* whilst giving it, and during the earlier inhalations must very carefully and *studiously adjust* the anæsthetic to the sensations of the patient. 13. A careful, attentive student, with tact, and not hard and unfeeling, can easily and in a short time be taught to give ether properly.—Since the adoption of Clover's inhaler, I have had singular freedom from anxiety about my anæsthetics—far greater freedom than in the previous period, when I had to depend on chloroform. Finally, I would say that this favourable opinion of ether is based upon my experience of its use by a series of very able administrators—some of the Leeds Infirmary, others whilst acting as my private clinical assistants. Speaking as a looker-on, rather than an administrator, I should say that the chief points in the right administration of ether are: first, to overcome the nervous dread of the patient by applying the mouthpiece only; then to turn on the ether gently, until the glottis becomes tolerant and the patient is slightly unconscious; lastly, to complete the anæsthesia rapidly. In advising beginners, I compare the regulation of the quantity of ether to the "curve of harmonic progression." (Mr. T. P. Teale, Brit. Med. Jour., March 11.)

Anæsthetic Mixtures.—One fluid ounce of pure chloroform, half a fluid ounce of pure ethylic ether, and half a fluid ounce of ethylic alcohol (sp. gr. ·795) is perhaps the most usual form of combination of these anæsthetic substances. The substitution of mythylene bichloride for the chloroform is an improvement, and Dr. Richardson in his early experiments in the introduction of methylene bichloride used it successfully in combination with absolute ethylic alcohol without ether; and with more success still when pure methylic was used instead of ethylic alcohol. His formulæ were: Methylene bichloride, one fluid ounce and a half; absolute ethylic alcohol, half a fluid ounce. Or, methylene bichloride, ten fluid drachms; absolute methylic alcohol, six fluid drachms. The last named mixture, though a little slower in action than methylene itself, is considerered by Dr. Richardson to be "the safest known anæsthetic when the methylic alcohol is absolutely pure." (Lancet, May 6.)

ADVANCED PHARMACY.—Dr. Walker, of Spondon, insists on the superiority and certain action of the alkaloids and essences of drugs, given at short intervals till their physiological action is produced. Nothing new is claimed for this method, for it has been advocated for some time past, but at present is little, or scarcely at all, resorted to by the general profession. It is only by constant dropping on the stone of the professional mind that any lasting impression is produced. Thus Sidney Ringer, in his "Therapeutics," recommends small and oft-repeated doses; as of aconite "in half-drop or a drop of the tincture in a teaspoonful of water every ten minutes or quarter of an hour for two hours and afterwards hourly;" lobelia "ten drops (of the simple tincture) every ten minutes or quarter of an hour till the dyspnœa gives way. It is better to adopt the smaller and more frequent dose, as the medicine can be discontinued should sickness or depression occur;" camphor, spirits of chloroform, every five or ten minutes in colic; chloral, in fifteen-grain doses every quarter of an hour till sleep is produced; alum, every ten or fifteen minutes (in croup) till vomiting is induced; perchloride of iron (in diphtheria) every hour; ipecacuanha (in dysenteric diarrhœa of children) drop doses hourly. Why should we on being called to a patient wait for the malady to declare itself, or that we may make a nice diagnosis, before we commence active treatment? Say that a rigor has ushered in an acute illness—inflammation, fever, or what not—and there are already a quickened pulse and ascending temperature. We know these are hurtful, exhaust vitality, and tend to adynamy. Then why wait till crepitation, rose spots, or other signs or symptoms warrant an accurate diagnosis, and why stand useless onlookers, giving some simple *placebo* to satisfy the patient or his friends, when you can be doing good and doughty service in attacking the pyrexia and bringing down the temperature and quickened pulse by the defervescent alkaloids, aconitine, veratrine, digitaline, quinine (some or all of them) every half hour or hour, until the thermometer or other sign bids you hold your hand? It will be advanced that you cannot trust dangerous remedies to ignorant attendants. True, if you give these remedies at very frequent intervals you must be guided by more than the appearance of the patient or the feeling of the skin. It is necessary to take the temperature at frequent intervals, say every two hours; and when it approaches the normal to slacken or discontinue the defervescents. There is no difficulty in instructing even a poor cottager in so simple a matter, at least I have never found any. And these are rare and exceptional cases, demanding anxious care and vigorous treatment. In this way many a threatened severe illness may be made to abort or run a comparatively benign course. Let me illustrate this by an actual case. A youth aged twenty was attacked with severe rigors, alternating with flushings of heat. He walked to me, a distance of a mile, when he was in a highly febrile condition, with dry skin, tongue, and lips, a pulse of 120, and a sharp pain in the right side. This was the fourth day of being unwell. Ordered to bed; to take an aperient, and $\frac{1}{120}$gr. of aconitine every hour. Seen next morning. Fifth day, 10 a.m.: Pulse 118, respiration 40, temperature 103°. Pain in side; cough; bowels moved several times. To take $\frac{1}{120}$gr. each of aconitine, veratrine, and arseniate of strychnine half-hourly.—8 p.m.: Pulse 106; respiration 28, temperature 102°. Acute pain in side; cough; tenacious sputum, with blood; tongue parched. To continue treatment. Sixth day, 10 a.m.: Pulse 96, temperature 101°. Sputum rusty and tenacious. To contine treatment.—8 p.m.; Pulse 94, respiration 28, temperature 102·2°. Two motions. To take one quart of milk, with two egg yelks, and mutton broth. Seventh day, 10 a.m.: Pulse 102, temperature 103·4°. Two motions. The temperature had gone up since last night, and there was some delirium. To give remedies every quarter of an hour. Thermometer left, and temperature to be taken every two hours.—8 p.m.: Pulse 62, respiration 35, temperature 100°. Aconitine and veratrine suspended, and strychnine also, as some slight twitching was noticed. To take arseniate of quinine every half hour, and one ounce of port wine with one egg yelk, and half a pint of milk every three hours. Eighth day, 10.30 a.m.: Pulse 72, respiration 28, temperature 98·6°. Restful night, though not much sleep. To continue treatment.—8 p.m.: Pulse 74, respiration 30, temperature 98·2°. Ninth day: Progressing favourably. Asking for food. Tenth day: Sitting up. (Dr. Benjamin Walker, Lancet, Dec. 10.)

INHALATIONS.—*Formulæ and Quantities for.*—Amyl nitrite, pure for adults; one in ten of the tincture for children. Benzole, rectified, used undiluted; Bromine, three grains (i.e., ♏ j) in 299 minims of distilled water. Camphor, the B. P. spirit. Iodine, the P.B. vapor. Iodoform used pure. Kreosote deodorised, used undiluted. Mephitis putorius, one in ten of the tincture. Musk, one in ten of the tincture. Phenol, absolute, ten grains to one ounce of boiling water. Oleum pini, all the pine oils, including terebene, also eucalyptus, used pure. Soda chlorata, the P.B. liquor. Stramonium, used by burning the powder or the P.B. tincture as a spray. Sulphurous acid, the saturated solution, or by burning brimstone. Thymol, fifteen grains to one ounce. Belladonna, drosera, ipecacuanha, not being volatile, used as a spray. I do not like sponge, because it tempts people to employ the same piece repeatedly. It thus stores disease-germs, and also ceases to be permeable. A better plan is to lay a thin stratum of well pulled absorbent wool in the cap, and then deposit on it about five minims of liquid, or about twenty grains of solid, of the material to be inhaled. Over this put another thin layer of absorbent wool, then replace the cap and adjust it to the face. The drug should be renewed every half hour, fresh wool being employed each time. The inhaler should be used all day if possible. At night I frequently direct the patient, if likely to be disturbed, to wear the respirator with some appropriate sedative, and to go to sleep with it on. (Mr. Edward Blake, Lancet, May 6.)

NEW GAS-CAUTERY.—At a meeting of the Surgical Society of Ireland, Dr. Beverley Cole, of San Francisco, exhibited a gas-cautery of his invention, which he regarded as a great improvement on Paquelin's instrument. It consisted of a metal tube, to one end of which were attached two India-rubber tubes—one for gas, which could be attached to any ordinary gas-burner; the other conveying the air from a reservoir of India-rubber charged, before the operation, by a hand-bag like that of the ordinary ether spray. The other end of the metal tube was fitted with knife, button, or rod of platinum, as required for ordinary external cauterisation, or for cutting operations. To make the instrument available for intravaginal uses, the handle was fixed to the tube by a hinge-joint, so that it could be set at any angle required. The relative and absolute quantities of gas and air were regulated by means of screws, and could be arranged to give any desired degree of heat. (Brit. Med. Jour., Dec. 17.)

VEGETABLE ASTRINGENTS.—Dr. Lewin recommends a new preparation of tannin. Experience teaches that solution of tannin, and to a still greater extent tannin in the form of powder, often fails to effect the purpose for which it is administered, and gives rise instead to secondary effects referable to its stimulating action upon the alimentary canal. Thus it causes a sensation of pressure in the epigastrium, loss of appetite, furred tongue, and diarrhœa. These inconveniences are lessened if the tannin be given in the form of a soluble albuminate according to the following receipt:—To a 1 to 2 per cent. solution of tannin, add a filtered solution of the white of an egg in 100 cc. of water, and shake the two well together. An opalescent liquid will be obtained which is more weakly astringent to the taste than a pure solution of tannin of the same strength. The prescription might run as follows:—℞. Sol. acidi tannici (2 p. c.), 100; adde agitando sol. albuminis ovi unius, 100. This solution diluted to a proper extent has been used with good results for children of only a few weeks old. (Practitioner, Feb.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE INTIMATE NATURE OF ZYMOTIC POISONS.

By JOHN DOUGALL, M.D., Lecturer on Materia Medica at the Glasgow Royal Infirmary School of Medicine.

What can be the nature of the potent agent that plays such terrible havoc with human, and it may be added, with lower animal life? An agent that causes an amount of mortality yearly, in Britain alone, not probably surpassed by all the battlefields of Europe in twenty years. An agent which spares neither the learned nor illiterate, the rich nor poor, the young nor old. An agent which often shows us how contemptible and impotent are all our powders and potions, and numerous devices, to thwart, in the smallest degree, its dogged course and fatal issues. An agent which, in the subtlest manner and most attenuated form, steals into the blood, always contaminating its pure current, and frequently tainting it to death by its keen virulence. Of course, it is obvious that it cannot be any, even of the most active, poisons known to the chemist or toxicologist. It cannot, *e.g.*, be phosphorus, arsenic, strychnia, atropia, aconitia, prussic acid, &c., &c.—that is clearly quite out of the question. What, then, can it be? Is it a vegetable, animal, or mineral product? Is it solid, liquid, or gaseous? Does it taste sweet, sour, bitter, salt, or acrid, or is it tasteless? Is its odour pleasant or offensive, or is it odourless? Is it opaque or transparent, coloured or colourless, amorphous or crystalline, acid, alkaline, or neutral? Can it be produced synthetically, and if so, how, and from what? Is it a simple or complex body? Has it any known reactions with chemical agents by which it can be isolated, or other distinctive properties by which it can be known, or is it only recognised by the manner in which it perverts the healthy functions, or produces death? When, and by whom was it discovered? Why is it called "zymotic poison"? Has it any antidotes? In short, has it been seen at all? Such are the chief questions in regard to any alleged lethal substance which naturally occur to those versed in the chemistry and toxicology of the more common poisons. But, at present, I cannot do more than answer these in a very general manner, and without regard to the order in which I have put them.

Zymotic poison, then, in no way resembles any other toxic substance, excepting that it is, as already indicated, "a deadly poison." It differs, on the other hand, from all other such bodies, in respect that it cannot be confined in stoppered bottles, measured in minims, nor weighed in grains; and, in particular, it differs in this important point that, while the actions of other poisons are confined to the individual affected, it renders the person it poisons poisonous. For example, as you know, one may attend closely on a patient suffering from a fatal dose of opium, or strychnia, without danger of becoming narcotised or tetanised by the opium or strychnia which the patient has swallowed. But one who has not survived the special form of zymotic poisoning with which a person is affected, could not attend him with impunity. Because he might poison his attendant, his nurse or doctor; nay, even his friends or neighbours in the same house or land, with the identical virus by which he himself is poisoned. His body is, for the time being, an alembic in which a special virus is elaborating and multiplying enormously, by an apparent destructive distillation of the blood and secretions, so that the tissues may literally be soaked, and the body enveloped in an atmosphere of virulent infection. The blood, saliva, breath, sweat, urine, fæces, and epidermis may all be saturated with it. This poison is named from the Greek word ζύμη, or leaven, and a person under its influence is held to be in a condition of zymosis or fermentation. It was so denominated because, when it enters the blood of an individual susceptible to its influence, it gives rise to phenomena very analogous to those caused by the addition of yeast to a substance capable of fermentive change.

Supposing, for example, there is added to an aqueous solution of honey, or of grape or other sweet fruit juice, a single cell of the yeast plant, *torula cerevisiæ.* This speck of matter is about the size of a human red blood corpuscle, and hence only visible under the microscope. Supposing, further, that this solution is kept at a temperature of about 70° Fah., then in a few hours, or at most a day or two, from the single yeast cell millions are produced, so as to make the liquid turbid, and form a frothy scum at the top, and a sediment at the bottom. During these changes the temperature of the fluid is increased, and carbonic anhydride largely evolved, until latterly the heat begins to return to the normal, the evolution of gas to cease, and the solution is now found to have lost its sweet taste, and changed into a mixture of alcohol and water incapable of further fermentation.

Now for the analogy between this process and that of zymotic poisoning. Supposing I were to wet the point of a needle with lymph from the body of a small-pox patient, and push it under the skin of a person who had neither been vaccinated nor suffered from small-pox. Then a thousand chances to one that, in a few days, perhaps hours, the recipient of the lymph feels out of sorts, has nausea, vomiting, headache, thirst, backache, a high pulse, and hot skin. In about three days after, a papular eruption appears on his body, which soon changes into small blebs or vesicles. These may be few or many, separate, or confluent, but the clear, and apparently homogeneous fluid which they contain has the identical specific infective nature as that with which the needle was wetted, the minute portion on the needle having now multiplied in the blood of the person inoculated to an enormous extent. Now, I need scarcely say that the symptoms just enumerated are those of small-pox, and that the lymph introduced into the blood by the needle is zymotic poison.

The putting of the yeast cell into the sweet solution, and of the minute portion of small-pox lymph into the human body, are both cases of pure infection, and the zymotic phenomena evoked thereby are almost perfectly analogous.

You observed 1st. That the saccharine solution was susceptible to the action of the yeast, and the human blood to that of the lymph. 2nd. The extremely minute portions of yeast and of lymph employed. 3rd. The rise of temperature in both cases; the sweet solution becomes abnormally warm; the small-pox patient highly fevered. 4th. The great multiplication of yeast in the sweet solution, and of small-pox lymph in the blood. 5th. The yeast frothing to the surface of the fluid, and the small-pox eruption forming lymph bubbles, or vesicles on the skin. 6th. Both are infectious; a cell of the new yeast would cause fermentation in a susceptible fluid; a speck of the new lymph would cause small-pox in a susceptible person. 7th. The sweet solution is no longer capable of fermentation, from the chemical change it has undergone in fermenting; the small-pox patient is no longer susceptible to small-pox, also from some chemical change produced in the body by that poison. 8th. The sweet solution will ferment, as it were, spontaneously—that is, without putting yeast cells into it intentionally, because such cells are constantly floating about, and fall on its surface; so may a person take small-pox spontaneously by inhaling air containing that specific poison. These are both cases of pure infection, though in neither is the infecting agent visible. Now I have chosen small-pox to compare with fermentation, because it is a typical species of the genus zymotica, the other chief members of which are—scarlatina, measles, diphtheria, typhus and enteric fever, and cholera. These affections are generically related in respect that they are caused by a something which either enters the blood from without, or is, as seems in some cases, generated therein spontaneously, and reproduces itself to an enormous degree, usually rendering the secretions and excretions of the affected person poisonous if inhaled or swallowed by a healthy person susceptible to their action. Also, that they commonly only make one attack, and always cause a high pulse, and increased bodily temperature.

Their specific differences are so remarkable and distinct, that each may be fairly considered as caused by a separate form of virus. For example, a medical man may be called to see a patient who sickened two days before, and now complains of thirst and headache; is fevered, prostrate, the eyes suffused, the tongue probably dry, furred, and studded with red points; the throat swollen and tender, and the skin covered with an exuberant efflorescence of bright scarlet dots, and he pronounces the case scarlatina. Or the patient may have swelling of the eyelids, with eyes red, watering, and intolerant of light, sneezing, dry cough and hoarseness, with difficulty of breathing. On the fourth day of the fever, the skin gets covered with small red dots, which coalesce into slightly raised crescentic blotches, and the case is diagnosed as measles. Or the patient may have stiffness about the neck, slight diarrhœa, nausea, pain on swallowing, salivation, fetid breath, the throat covered with specks or patches like damp, dirty, wash leather, symptoms indicating diphtheria. Or the patient may have frequent chills, a soft pulse; dry, brown tongue; constipated bowels; heavy, flushed, smoky countenance; stupor; prostration; wakefulness; delirium. About the seventh day of these symptoms the skin is covered with irregular spots of a dusky or mulberry hue, which at first disappear on pressure; are slightly elevated, but soon become persistent—symptoms showing the case to be typhus fever. Or the patient may have pains in the limbs, great weakness, loose bowels, with very offensive stools, which may contain blood; glazed dry tongue, with ammoniacal breath; a pale languid face, with a hectic flush on each cheek; clear eyes, with dilated pupils, and the mental faculties unaffected. About the eighth day a few elevated and isolated rose coloured dots are probably found on the abdomen or chest, vanishing on pressure, appearing in successive crops, which last two or more days, symptoms clearly indicating enteric fever. Or, lastly, the patient, quite well a few hours before, may have severe diarrhœa, with evacuations like rice water, cramps in the limbs, causing the muscles to be hard as wood, or contracted into knot-like masses; the circulation and respiration impeded; the nose, tongue, ears, breath, and extremities icy cold; the lips and general surface blue or livid;

the eyes sunk; the pupils contracted and fixed; the complexion muddy and the features pinched; the whole body shrunk and intensely prostrate; perhaps death taking place before the doctor has time to prescribe for what is too obviously a case of Asiatic cholera.

What is zymotic poison? I may at once say that this problem has hitherto all but baffled the skill and eluded the research of the most profound thinkers, and patient investigators in medical science.

There are two theories regarding the nature of zymotic poison—viz., the "germ theory" and the physico-chemical theory. Believers in the germ theory (which theory is of very ancient date) hold that zymotic diseases are caused by microscopic living organisms or germs which, entering the blood, set up morbid or zymotic changes, characteristic of the special organisms, which may be scarlet fever germs, small-pox germs, cholera germs, &c., &c., each infectious disease having its special organism, germ, or ferment. The germ theorists also affirm that putrefaction is caused by germs which, they allege, in densely populated places especially, are floating in myriads in the atmosphere, and only need a congenial soil, such as dead organic matter, or a weakly organism, to make them reproductive. Hence germs require morbid conditions for their existence. Now, in order that you may fairly comprehend the theory, I must say a few words on putrefaction. If a portion of clear fresh urine, blood serum, or juice of flesh, be freely exposed to the air for a day or two, at a temperature of about 70° Fah., and then a drop of it examined by the microscope, it will be found swarming with organisms called bacteria, measuring from about 1-40,000th to 1-100,000th of an inch. The fluid is now turbid, fetid, neutral, or alkaline, but never acid. On the other hand, if portions of any of these liquids be boiled in separate glass flasks for about a minute, and during ebullition the necks of the flasks stuffed with cotton wool, and the steam allowed to issue through it a short time, and if the flasks be now set aside, their contents will be found, months or years after, pure and unchanged. But if at any time the cotton be removed from one of the flasks, then in a day or two its contents will putrefy, while those of the other flask remain fresh and clear.

Now, these experiments show that it is not the air which causes putrefaction, because the cotton wool is pervious to air; neither can the cotton obviously have any antiseptic effect on the fluid, nor chemical action on the air passing through it into the flask. Nor can it be the boiling of the fluid which keeps it fresh, as such fluids, in ordinary circumstances, soon putrefy after boiling. This process is resorted to for the purpose of destroying *that* in the fluid which would cause it to putrefy were it not boiled, even although the neck of the flask were stuffed with cotton wool. The cause of putrefaction must hence be something in, but distinct from, the air, and this something is obviously filtered by the cotton wool from the air entering the flasks. Now, according to the germ theorists, the something consists of the germs of bacteria and their allies, vibriones and monads, but chiefly of bacteria, which swarm in all putrid liquids. This, however, must be considered a mere inference, as no power of the microscope can detect the germs of adult organisms so minute as the 1-40,000th of an inch, if they bear, which they doubtless do, the same relation in size to the parent organism, as in most other cases either in the vegetable or animal kingdom. Compare, *e.g.*, the minute nautilus-like seed of the opium poppy, 200 of which only weigh a grain, to the entire plant, and its numerous capsules as large as oranges; or compare an acorn to an oak; or the human germ, measuring about 1-250th of an inch, to a man weighing 16 stones; or the germ of the bull and horse, which are said to be even smaller than that of man, to the adult animals.

The origin of these micro-organisms was, a few years ago, made the subject of numerous interesting experiments by Dr. Bastian, of London, who asserts that he has thereby demonstrated that life may arise anew from dead matter. I shall not dwell on this point, but merely state that Bastian obtained results which seemed to prove that life arose in certain rich nitrogenous fluids after they had been heated from 260° to 302° Fah., while hermetically sealed in glass flasks—a temperature I am convinced, however his results may be explained, that no living thing, germ or other, vegetable or animal, could resist.

The fact however remains, that minute organisms teem in all putrefying solutions, whatever way they get there, and are absent in all solutions not putrefying, whatever way they don't get there; and while the germ theorists assert that they are the cause of putrefaction, and that their functions in nature are malignant, the physico-chemical theorists generally believe that they are the result of putrefaction, and that their functions in nature are beneficent. The chief points adduced by the germ theorists in support of the malignant function of such minute organisms are—1st. That they are present in all putrid, and absent in all fresh or healthy organic matter. 2nd. That many diseases, both of animals and plants, they allege, are caused by parasites and fungi. The opponents of this theory, of which I am one, hold that all such organisms are the result of the morbid conditions of their habitat. 1st. Because that whether bacteria, parasites, or fungi, they are only found on the parts of animals or vegetables with lowered vitality, or in their dead tissues. 2nd. That by strengthening the vitality in living, or arresting decay in dead parts, they disappear. 3rd. That when bacteria appear in infectious matter it has lost its power to infect, as observed in small-pox and vaccine virus. On these grounds, the functions of such organisms are held as beneficent. Professor Owen, with that large grasp of intellect which marks all he says, calls them "nature's scavengers" for maintaining the salubrity of our atmosphere, and "nature's invisible police" for arresting the fugitive organised particles, and turning them back into the ascending stream of animal life.

As regards the physico-chemical theory, this was first propounded by Liebig, and consists in holding that "putrefaction is a transformation of the atoms of a compound yielding two or more new groups, and caused by contact with other substances, the atoms of which are themselves in a state of transformation or decomposition." Hence, according to this view, that which is intercepted by the cotton wool of the flasks is not living germs, but particles of dead matter in a state of transformation or decomposition. Now, such particles are found everywhere near the earth's surface. In a sunbeam entering a dark room, through a hole in the shutter, myriads of floating motes are seen, which have been proved organic. Professor Tyndall has reproduced this phenomenon very successfully. A strong beam of electric light is shot across a darkened space which shows the air to be a mere stir about of very small particles. If a person breathe into this beam the particles are much agitated and increased; but, if the breath be blown through a tube covered with cotton wool, a black band cuts the beam in two where the breath meets it. This is caused by the cotton filtering the aqueous vapour of the lungs and the particles of organic matter from the air which enters the beam at the black band. Or if a red hot iron ball be held under the beam, then a current of zig-zag, inky blots cuts through it, in consequence of the moist and organic particles being rarified and burnt by the heat of the ball. Tyndall has satisfactorily demonstrated that, optically, pure air is black, and the transient bands and blots in the electric beam are glimpses of such. I have often examined, by the microscope, cotton wool which had filtered air from such particles, and always found a very few bodies resembling sporules and cells; but the mass in every instance consisted almost exclusively of shapeless, ragged morsels of various sizes, having no signs of organisation, but every appearance of organic debris. These particles, then, are obviously what enters the liquid in an open flask, causing it to decompose, thus strongly confirming Liebig's views. The living organisms, also present in putrescent fluids, the physico-chemical theorists say, as already mentioned, are mere harmless concomitants, having no causal connection with their medium, and that their life conditions are in reality not matters for the pathologist at all, but for the naturalist. Moreover, the adherents of this theory likewise hold that not only is putrefaction caused by contact with minute particles of putrid matter, but that this is also the mode of multiplication and the form assumed by zymotic poison, whether it come from the lungs, skin, or bowels, &c., &c. What the proximate chemical composition of these zymotic particles is, can only be conjectured; they have been thought, however, to be carbo-ammoniacal. The physico-chemical theorists concede, of course, that each communicable disease has its special poison, but deny that the poison causes disease by an inherent self-reproductive power. Zymosis, they consider, is continued from person to person, by the infectious virus imparting its condition to those who contain in themselves that which is capable of being changed into the poison.

The most feasible enunciation of the physico-chemical theory is that of Dr. B. W. Richardson, who says, "Zymotic poisons are derived from the secretions of the body, so that a zymotic patient is poisonous precisely as a *cobra di capello* is poisonous. He is secreting virus which, by contact with a healthy person, may cause disease. As each healthy secretion has specific properties, so each when diseased acts as a poison, and, from its local position when so acting, local symptoms arise characteristic of the disease. There is thus a different secretion forming a pabulum for each poison—one for small-pox, one for scarlatina, and so on, the one virus not thriving on the food of its neighbour, but electing its own secretion." Further, he says, "According to the germ theory, each zymotic case yields an enormous number of germs, all of which retain their vitality amid such extreme temperatures, that the wonder is the world is not depopulated by them. But the physico-chemical theory places zymotic virus under the same atmospheric influences as other organic matter, whereby it is soon reduced to elementary forms. Hence the dead are not contagious and epidemics cease."

Dr. Richardson also considers, and I agree with him, that zymotic poison may, under extreme mal-hygienic states, arise *de novo* in the secretions, from which effects follow precisely as though it had been introduced into the body, and attributes the immunity from, and tendency to, infectious disease at various ages, to a constitutional or physiological peculiarity of secretion; and the non-recurrence of the same disease in the same person to the elimination of particles in a secretion susceptible of zymosis. Moreover, he says, "I protest that the attempt to connect our knowledge of the minute organisms with the phenomena of the various communicable diseases so as to suggest, or as some do, to assert, that the diseases in question arise from germs, and that the person affected with a contagious disease has been fertilised like a piece of ploughed land, or virgin soil with a crop of germs, and in turn he is the soil in which another crop is being produced by the independent increasing or multiplying of the germs in him, I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive. What most astounds me is that men who are conversant with the practice of physic, who are treating diseases of a communicable kind every day, should for a moment connect such a hypothesis with the phenomena they have under their observation. Does any one of them believe that hydrophobia is from a germ, that syphilis is from a germ, or other diseases I need not specify?"

But, whether zymotic poison be living germ, or putrid speck, if it be at all tangible, what does the microscope or test tube say about it? As already indicated, this problem is not only one of the most interesting in medical science, but also one of the most complex that the mind can grapple with. Although of late years diligent search has been, and is being, made by the highest qualified persons, and with the best apparatus procurable, for the different human zymotic poisons, yet, in reality, only one has been found—viz., that of small-pox. This virus is easily obtained pure from a small-pox patient during the vesicular stage of the eruption. Like vaccine lymph it is fluid, translucent, odourless, alkaline, and albuminous, hence coagulable by heat and acids. Under the microscope both are found to contain numerous black, roundish, motionless particles, about the 1-20,000th of an inch in diameter. M. Chauveau, a distinguished French veterinary pathologist, first, and Dr. Burdon Sanderson, of London, latterly, succeeded, by an ingenious method, in separating the particles from the fluid parts. After this they found that a child might be vaccinated or inoculated with the fluids without effect, while the minutest portion that can be separated of the solid parts causes all the phenomena of cow-pock or small-pox. Now, the question comes to this, are these black, virulent specks, motionless organisms, or dead putrid particles? This is yet unanswered, although the germ theorists consider them organisms and call them micrococci. So also is the query, How do the particles of cow-pock lymph, as in vaccination, prevent the virulence of those of small-pox lymph? And, further, are the specific viruses of scarlatina, typhoid fever, &c., &c., of the same form? Any answer to the latter question can only be a mere surmise founded on inference, for the simple reason that these viruses have never been seen.

Some distinguished pathologists in France, Germany, and Britain, seem to have their minds made up that all communicable diseases, and many of an inflammatory nature, of man and the lower animals, are caused by minute living organisms.

I admit that presently the germ theory seems most in favour, and if its adherents proceed as they are doing, we shall soon have disease germs enough, these being apparently blamed as the cause of every pathological state where they are found, and that is in every pathological specimen, animal and vegetable; and moreover, for many diseases in which they are not found, and they are always looked on, by the germ theorists, as the cause of these states and diseases, but never as the result.

While speaking thus of the germ theorists, it is far from my desire to disparage their arduous and interesting labours. On the other hand, the brilliant and patient researches of Pasteur, Klebs, Koch and others, in France and Germany, and of Sanderson, Lister, Roberts, Ogston, &c., in Britain, command the admiration of every purely scientific mind, and seem presaging results which, if realised, will be the means of preventing, both in man and the lower animals, an enormous amount of disease, and an immense number of deaths. At the same time, it appears to me that these distinguished *savants*, in many of their investigations, tacitly admit the physico-chemical theory, while ostensibly discarding it. Also that probably all, at least certainly most, of their results already obtained may be as satisfactorily interpreted, and those anticipated as easily realised, on the physico-chemical, as on the germ theory.

Such are a few of the chief facts and considerations regarding the nature of zymotic poison.—*Glasgow Medical Journal, Nov.* 1881, *p.* 325.

2.—ON SEPTICÆMIA, AND THE TRANSFORMATIONS OF BACTERIA.

By the Editor of the Lancet.

A series of experiments, which have been carried on at Wurzburg by Dr. Rosenberger, seem to involve important modifications in the views at present current regarding the development of septicæmia and of the organisms to which it is in apparent relation. It has already been found that a septicæmic virus, which has been exposed to a temperature sufficient to destroy all the bacteria it contains, will still cause the death of an animal into which it is injected. It is currently assumed that the animal is poisoned, not infected—*i.e.*, that the virus causes death by its presence, not by its multiplication, and that a second animal cannot be killed by the injection of a small quantity of the blood or serum of the first, any more than in a parallel case of strychnine poisoning. To decide this question, septic blood and serum were heated, filtered, evaporated, and then injected. The animals died with all the symptoms and pathological appearances of septicæmia, just as if the uncooked virus had been used. Moreover, it was found that the blood-serum of the tissues and cavities contained the same micro-organisms and in the same quantity as animals into which the uncooked virus had been injected. The effect of heat was, however, to lessen greatly the virulence of the poison, so that a cubic centimetre was needed of a virus of which before the minutest quantity sufficed. Further experiments showed that septicæmia could be produced in other animals by the injection of even extremely small quantities of the blood- or tissue-serum of those that had been killed by the inoculation with the cooked virus, which appeared thus to have acquired by renewed development the intensity of which it had been deprived by heat.

An obvious objection to the experiments of Rosenberger is that the heat employed may have been insufficient in degree or in duration, and that some of the specific micro-organisms may have escaped its action. To meet this objection he exposed the virus to a temperature of 140° C. for two hours, and made the inoculation with a perfectly new syringe washed with carbolic acid and absolute alcohol, but obtained the same result. The fact that the virus was effectually sterilised, so far as organisms are concerned, was ascertained in each experiment by the addition of some of the virus to some extract of meat, which remained for weeks perfectly clear and free from organisms.

The inference from these experiments is that the application of a degree of heat which apparently sterilises effectually a septicæmic virus, so far as artificial cultivation of the organisms is concerned, does not prevent the virus from producing in the animal body its specific form of septicæmia and of septic bacteria. From these facts the startling conclusion is drawn that the bacteria are not primary but secondary elements in the morbid process, and that their development is associated with a chemical or at least unorganised poison. It is not asserted that the bacteria are without influence on the morbid process; on the contrary, Rosenberger believes that they alone are the means of multiplying the poison in the animal body, and in a culture fluid inoculated with the cooked virus and kept at a suitable temperature there is not only no growth of organisms, but also no increase of virus. If two similar culture fluids are inoculated, one with the cooked virus and the other with some blood of an animal which has been killed by injection of cooked virus, the former remains unchanged, and injected into an animal is without effect, while the other becomes opaque in the course of a few hours, and has developed powerful septic properties.

Important and suggestive as are the experiments of Rosenberger, the conclusions which he draws from them are so much opposed to those of most previous experiments, that to many persons the most probable explanation will doubtless be the assumption that some germs must have resisted the action of heat, and have survived in a condition to develop in the favourable soil of a living organism, although not in a mere culture liquid. At the same time it must not be forgotten that septicæmia belongs to a class of diseases of low specificity, like typhoid fever, diphtheria, and erysipelas, the occasional origin of which by the transformation of a non-specific virus is highly probable.—*Lancet, Feb.* 18, 1882, *p.* 279.

3.—ON THE BACILLUS OF TYPHOID FEVER.

By the Editor of the British Medical Journal.

Whether infectious diseases depend on the development of micro-organisms in the living body, may be considered to be the great question of modern pathology. Notwithstanding the difficulties attending the investigations to which it leads, this question is being answered in the affirmative by a constantly increasing number of investigators. Micro-organisms, and more especially the so-called cocci or spheroids, are so minute and so difficult to distinguish from other accidental granules of various kinds, that their alleged existence in a tissue or a fluid may usually be received with a certain degree of scepticism. Dr. Creighton's demonstration that products of coagulation in some cases had undoubtedly been mistaken for bacteria, gave strong encouragement to this scepticism, and in this country, for a time, probably gave a check, if not to the progress of investigation in this direction, yet to the publication of its results. But the facts clearly ascertained, and above all the complete knowledge which had been acquired regarding the bacillus of anthrax, were too striking and too suggestive to permit Dr. Creighton's criticism to have more than the legitimate effect of showing the necessity for extreme care and caution. If products of coagulation are liable to be mistaken for bacteria, that is no reason why it is to be concluded that bacteria may not be the active *contagia* of disease, although it is a good reason why observations which seem to point in this direction should be carefully scrutinised.

But when it has been shown beyond cavil that bacteria do exist in the living body in certain contagious diseases, it still remains to show that their presence is intimately associated with the existence of a specific disease. This is a branch of inquiry that at present engages many experienced observers; and, although it involves problems which are difficult to solve satisfactorily, we seem at last to be approaching solid ground in another of the diseases in which it has been claimed that a specific malady is associated with the development of an organism. Prof. Eberth, of Zürich, has recently published a series of observations which strongly corroborate the statements with which, in this country, we are familiar, regarding the dependence of typhoid fever on the development of a micro-organism. In the 81st volume of Virchow's Archiv, he has shown that in this disease the intestinal mucous membrane, the mesenteric glands, and the spleen, contain rod-bacteria, differing, as he believes, from organisms found in the body in other conditions. In a recent number of the Archiv (Vol. 83, 3 Heft) he adduces further evidence to the same effect, the observations being controlled by examinations of these tissues in cases of death from other diseases. In eleven such cases, including deaths from various septic processes, the bacilli of typhoid fever were not found; nor were they found in thirteen cases of phthisis, in some of which there was extensive ulceration of the intestinal mucous membrane.

Eberth's later paper reports on seventeen cases of typhoid. The bacilli which he regards as distinctive were found in six cases, and were missed in eleven. In the six cases the number of bacilli was in inverse proportion to the duration of the disease. In the case of shortest duration, eleven days, it is noted that in the mesenteric glands, in each section, ten to twenty masses of bacilli were found, sections from the spleen also containing them in large numbers; whilst in that of longest duration, forty-three days, none were found in sections from the spleen, and in fifty sections from the mesenteric glands only two masses of bacilli were observed.

These bacilli appear in shape and size not to differ from ordinary rod-bacteria; but Eberth believes that they differ from them in their small capacity for taking on the staining of hæmatoxylon, methyl-violet, and Bismark-brown.

On the other hand, eleven cases are given in which no bacilli were found: a negative result of the greatest importance, which, if we accept Eberth's theory, it is difficult to understand.

It is impossible to over-estimate the importance of these investigations. The subject is doubtless one which is still occupying competent investigators. Prof. Eberth's paper is calculated to stimulate workers in our own country who have already distinguished themselves in this field, to continue their observations in reference to typhoid, under the safeguards which more extended experience in the study of bacteria has made available.—*British Medical Journal, Nov.* 26, 1881, *p.* 877.

4.—ON FERMENTATION AND DISEASE.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

The relation between the action of micro-organisms on suitable fermentescible materials, and their effects when introduced into the living body, is one of the most important aspects of the great question of the bacteric origin of disease; but, owing to the great difficulty of the problem, comparatively little has been made out regarding it. Dr. Cameron has given an excellent *résumé* of the principal facts at present known, in an address which he delivered before the Glasgow Philosophical Society on December 14th.

Dr. Cameron deals chiefly with two of the analogies between the action of these ferments in fluids and tissues and in the living body. The first of these is, that a similar association may be observed between the presence of micro-organisms and the occurrence of fermentation as is now known regarding their relations to various diseases. That micro-organisms are the causes of fermentation, with the exception of some cases which are really instances of double decomposition or merely chemical phenomena, has now been thoroughly established. The instance of the yeast-plant converting sugar into alcohol and carbonic acid is especially brought forward by Dr. Cameron, and is the best known case, though not by any means the only one of which complete proof has been furnished. Anything which interferes with the growth of the yeast-plant interferes with the occurrence of the alcoholic fermentation. Heat destroys the torula; heat arrests the fermentation. Pound the cells in a mortar so as to destroy them, and the formation of alcohol will not follow the introduction of the bruised yeast. Retard the growth of the plant by cold, and the alcoholic fermentation is retarded; aid its development by heat, and the chemical changes go on with correspondingly increased rapidity. The living cell is constantly present, and a vigorous and healthy condition of the plant is necessary for the proper occurrence of the fermentation. The lactic fermentation has like relations to a small oval bacterium, which has hence been called by Mr. Lister the *bacterium lactis*. The butyric fermentation, the decomposition of urea, the viscous change in fluids, and other fermentative processes, have been also traced to the agency of living cells belonging to the lowest forms of plant-life.

The demonstration that a fatal infective disease can be induced by the inoculation of micro-organisms on the healthy living body, has in like manner been clearly demonstrated in several instances, the chief of which is splenic fever. It has now been amply proved that the *Bacillus anthracis* found in the blood of the animals which have died of that disease is the active agent or cause of all the phenomena, and not merely accidentally present. Inoculate a healthy animal with a minute quantity of blood from another individual which has died of splenic fever, and the individual inoculated dies in a short time, its blood being full of the characteristic rods. Introduce the blood, with proper precautions against accidental contamination, into a suitable cultivating fluid, and this organism alone develops. Cultivate it outside the body for a series of generations, till no constituent of the original blood can possibly be present, and then inoculate the animal with this fluid, and again splenic fever is obtained in all its virulence, and with the presence of the same micro-organisms. Filter the blood or the cultivating fluid through plaster, and the liquid will be found to be innocuous; boil it long enough, and the same thing results. As in the alcoholic fermentation, so here, the presence and vitality of the plant are necessary for the development of the characteristic phenomena.

The analogy between fermentative processes and infective diseases is, perhaps, even more strikingly shown in the case of chicken-cholera, which has been thoroughly investigated by M. Pasteur. The organism which produces this disease is an oval bacterium, which, when unstained, is said by Pasteur to resemble a figure of 8. These bacteria have been cultivated outside the body, and the same facts have been made out with regard to them as we have just mentioned in the case of splenic fever. But it has been further shown that, while inside the body it flourishes, and in some way or other, as yet unknown, causes the death of the animal, it also, when grown in chicken-broth, causes fermentation and produces an alkaloid, in this resembling the other organisms which we have previously mentioned. Here, then, we have the two facts; viz., an organism growing outside the body causes a particular fermentative change in the medium in which it grows; the same organism, growing inside the living body, gives rise to certain changes which result in the death of the animal. In what way the fatal result is brought about—whether by the production of the same alkaloid as is formed outside, and which is poisonous to the animal, or by the removal of oxygen or of some necessary nutritious substance from the blood, or mechanically—is as yet quite undetermined. In the case of yeast, also, somewhat analogous facts have been made out. When the torula is grown in the blood outside the body, it deprives the red blood-corpuscles of their oxygen, and decomposes the sugar of the blood into alcohol and carbonic acid. If a sufficient quantity (more than two grammes) be introduced into the circulation, the result is always fatal; the chief *post-mortem* appearances being inflammation and ulceration of Peyer's patches.

The second point by which Dr. Cameron illustrates this analogy is the life-history of these organisms, especially their relation to oxygen. Here, however, he is entering on very debatable ground, the majority of the statements on this point being based on little more than theoretical considerations. The only instances which have a moderate basis in facts are the behaviour of the *Torula cerevisiæ* on the one hand, and of the organism causing chicken-cholera, and possibly also of the *Bacillus anthracis* on the other. The yeast-plant, when freely exposed to air, grows luxuriantly, but causes very little fermentative change in the wort. Diminish its supply of oxygen, and its fermentative power increases; and hence we have the view that it causes the alcoholic fermentation by abstracting oxygen from the chemical combinations in which it grows, provided that it cannot get sufficient free oxygen from the air. This view is apparently supported by the fact that other plants will cause alcoholic fermentation when similarly deprived of oxygen: and also that in ripening fruit, kept in an atmosphere of carbonic acid, the cells which are still active and require oxygen, produce alcohol apparently by the decomposition of the sugar which they have already produced. The experiments of Pasteur on chicken-cholera are well known. In the hope of diminishing the infective power of this organism, he grew it in oxygen for a long time, and then found, not only that it produced a modified disease, but that this attack in most cases protected the animal from the effects of the organism in its most virulent state. He therefore concluded, from analogy with the yeast-plant, that the result was due to the fact that the organism had been supplied with a large quantity of oxygen. The further extension of this principle to the *Bacillus anthracis* has led to the extensive adoption of protecting inoculation of animals. Before, however, we can accept this oxygen-theory, we must have many more facts, and the experiments must be repeated and extended by other observers.

In Dr. Cameron's attempted comparison between putrefaction and septicæmia, we fail to see the analogy. Though putrefaction be the complex process described by Dr. Cameron—and it is doubtful whether this be the correct description of that process—the forms of septicæmia produced by it are simple processes, each caused by its own organism. These organisms are, further, not essentially present in the putrefying materials; indeed, it is but rarely that they can be obtained from blood or other putrefying substances. The alleged increasing virulence of septicæmic blood, on which Dr. Cameron lays considerable stress, is not merely doubtful, but may now be looked on as entirely erroneous. We cannot enter into these and other interesting points raised by this paper; and it is the less necessary for us to do so at the present time, as we hope shortly to be able to publish a series of articles dealing with the most recent investigations on the subject of infective diseases. Sufficient has been said to show that a strong and undoubted analogy exists between fermentative changes and infective diseases.—*British Medical Journal, Dec.* 31, 1881, *p.* 1061.

5.—ON SOME NEW FACTS CONNECTED WITH THE ACTION OF GERMS IN THE PRODUCTION OF HUMAN DISEASES.

By Dr. George Harley, F.R.S.

In general it is easy enough to state medical facts, but not always equally easy satisfactorily to explain them; and as these eccentricities in the periods of incubation may be considered as belonging to the not easily explainable category, I must ask a little indulgence while attempting to make the matter plain. For, in order to avoid ambiguity, it will be necessary for me to digress a little, and begin by (1st) directing the reader's attention to the fact that the possession of a period of incubation before manifesting pathological activity is by no means the special attribute of living disease-germs. For it is a quality equally possessed, though in a much less striking manner, by almost every form of toxic agent, no matter whether it belong to the animal, vegetable, or mineral kingdom. Thus, for example, the most rapid of all known poisons, hydrocyanic acid, takes a few seconds to produce visible results, opium as many minutes, arsenic an equal number of hours. Here, however, the similitude between the living and the dead noxious agents ends. For, although syphilitic germs develop their constitutional effects in as many hours as hydrophobic ones take days, the rapidity or tardiness of the appearance of the symptoms is not, as in the case of dead deleterious agents, in direct proportion either to the quantity introduced or to the channel of their introduction. For, although we have it in our power to make a dead deleterious agent kill in a few seconds, or to retard its action for hours—strychnia, for example, when introduced into the jugular vein kills in thirty seconds. While the same quantity introduced along with the food into the digestive canal does not extinguish life at all. While a larger quantity takes from two or three hours to kill. With germs we have no such power. Introduce them into a blood-vessel, into the subcutaneous cellular tissue, into the lungs, or into the stomach, and yet they will take, with but little variation, the same time to produce their constitutional (not their primary local) effects. The element of quantity seems, however, to have some slight influence; but nothing in comparison with the element of quantity, as in the case of dead poisons.

2. Again, while not only the elements but the channels of introduction and the quantities introduced are important factors in the periods of incubation required by dead substances to produce their toxic effects, age and constitution have almost an equal influence. Thus, while a drop of tincture of opium will kill a new-born babe in the space of an hour, its mother will swallow a hundred of them with perfect impunity. A very different matter from the effects of pathogenic germs, whose effects and periods of incubation are almost identical in the aged and in the young.

One element alone, in the case of pathogenic germs, stands out in prominent grandeur. The element of predisposition. An element playing alike its part in the control of dead as well as living deleterious agents. But with this striking difference, that while in the case of the dead it usually plays a minor, in the case of the living it invariably plays the major, part.

From a careful consideration of the foregoing facts, it is seen that, although the modes of action in the production of the periods of latency of dead toxic agents and living animal germs are not identical, they are nevertheless apparently under the influence of the same common law. The discovery of the nature of this common law would no doubt be a potent lever in the elucidation of the point now under consideration; but as it involves the unearthing of a philosophic explanation of the causes of the periods of latency manifested by all kinds of dead toxic agents, its consideration is beyond the scope of this article. So I must leave it, and at once return to the special consideration of the causes giving rise to the different periods of incubation met with in human infectious and contagious diseases, and explain my ideas of the *rationale* of germ-action in this respect.

My explanation, I think, will be easily enough understood when it is known that it is founded upon the idea that it requires a multitude of every single given species of pathogenic germ to be able to produce human disease. One single germ not being potent enough to produce by itself a visible local or constitutional effect, any more than one single raindrop can by itself drive a water-mill wheel. 'Tis the aggregate of both which alone suffices—in the one case to produce disease, in the other to set in motion the water-mill.

Time, moreover, is an element equally required by both in order to enable them to manifest their power. In the case of the raindrop, for example, it is not alone necessary that it should fall to the earth and be there joined by a number of others, and collect into tiny tricklets, but the tricklets must unite together to form little streams, which in their turn must unite to form a watercourse of sufficient volume and rapidity of movement to turn the mill-wheel.

The time elapsing between the falling of the raindrops and their setting the mill-wheel in motion is their latent period of "power incubation."

I might perhaps have even chosen a better form of simile than raindrops, which would be this:—Supposing a boy let loose a pregnant rabbit on a little fertile island in a lake. At first the existence of its inhabitant concealed away in the grass and bushes would escape the notice of even a careful observer. A year afterwards, however, the first litter of the solitary doe would not only have arrived at maturity, but probably propagated its species four or five times over; and now the supply of the island's provender, from being but limited, and each rabbit possessing a famous appetite, would become exhausted. Consequently, not only would all the grass disappear from the island, but the bark of the bushes would also be consumed, and, as a natural consequence, they would cease to flourish, and the once luxuriantly healthy little island would be transformed into a barren waste. All that is required to make the simile complete is to look upon the luxuriant island as a healthy human being, and the rabbit as a pathogenic germ, *e.g.*, the barren waste as the diseased body, and the time occupied in the extinction of the island's vegetable fauna as the period of incubation of disease. In the same manner, then, as it required time for the raindrops to set the mill-wheel into visible motion, and the rabbits to convert the luxuriant little island into a barren waste, so in precisely like manner it requires the same elements of time for the inoculated germ to produce its visible effects in its human host. For it must have time given it to hatch a brood. The brood to develop into maturity. To reproduce its species, and to multiply sufficiently abundantly, either locally to produce a primary local lesion, or generally in the bloodvessels and lymphatics to induce constitutional disturbance.

The time thus required for the development of a sufficient number of germs in the human body to throw its working gear out of order and give rise to visible local signs or general constitutional symptoms, is what I regard as the incubation period of disease.

Next as regards my explanation of the *rationale* of the causes of the eccentric periods of incubation occasionally met with in cases of infection and contagion.

When I say that I believe that rational medicine is simply common sense on a scientific basis—that there is nothing different in the laws regulating the healthy and the diseased actions occurring in the human frame, from those regulating every other part of the universe and all that it contains, the reader will not feel surprised at my propounding my explanation of the causes of incubation's eccentricity in the form of the following three propositions:—

1. Notwithstanding the exceedingly minute size of pathogenic organisms, they belong either to the animal or vegetable kingdom, or are hybrids, and partake of a little of both.

2. Every species of animal, be it nude or clothed, with legs or without legs, large or small, as well as every species of plant, from the enormous *Wellingtonia gigantica* to the almost invisible lichen on the rock, develops, arrives at maturity, and propagates its kind with a speed in direct proportion to the suitability of its surroundings. When the surrounding circumstances are favourable, it multiplies rapidly; when the circumstances are unfavourable, slowly. So in like manner, we may suppose, do disease germs. When planted in a favourable host they develop rapidly; and in what we term a non-predisposed host, slowly.

3. As some of my readers may think that the above is scarcely a comprehensive enough explanation of the extraordinarily prolonged periods of incubation above cited, I must supplement it by reminding them that pathogenic germ-eggs, like the ova of insects, the spawn of fish and mushrooms, the seeds of plants and trees, may lie inactive, when in unfavourable conditions for development, for months or even years, and yet burst into activity when the conditions of their surroundings become suited to their requirements. It is a well-ascertained fact that common mushroom spawn will develop perfectly after having been kept stored up for several years. Added to which is an equally well-known fact that man has it within his power not only to retard or hasten the germination of the seeds of plants, but even the ova of animals. The eggs of insects, for example, placed under differently coloured pieces of glass, develop with different degrees of rapidity, and precisely the same thing I observed occurs with the spawn of the frog. The transformation of the chrysalis into the butterfly is hastened by heat and retarded by cold. The hatching of the spawn of the salmon follows an exactly similar law. Surely, then, we can have little difficulty in understanding why things like germs, possessing, as I shall afterwards show, an unusual amount of latent life—an amount almost equal to what is possessed by the seeds of plants—should lie inactive when the tissues of the human body are unsuited for their wants, and yet immediately burst into activity so soon as the conditions of their surroundings become favourable for their development. I shall say nothing more on this point at present, as when I come to

discuss the causes of periods of quiescence existing between epidemics of diseases, the latent life of germs will receive further elucidation.

The reader who doubts that the germ theory of disease explains the unusually extended abnormal periods of the incubation of contagious, infectious, and inoculable diseases, will please to do me the favour of deferring judgment until after he has perused what I shall say when attempting the explanation of the long intervals of quiescence which are occasionally observed between the cessation of one epidemic and the reappearance of another of a similar kind in the same locality; and I shall now pass on to the consideration of the causes of exceptional brevity in the periods of incubation.

About the cause of abnormally brief periods of incubation I think there can exist but trifling difficulty, as a satisfactory explanation of it is extractible from examples of immensely rapid germ-development, such as occurred in the case of an experiment of Pasteur's, which conclusively shows that pathogenic germs, when placed under favourable circumstances, develop not only into hundreds and thousands, but millions, in the brief space of a few hours. This was well and beautifully incidentally illustrated, in so far as the point before us is concerned, by Pasteur in his experiments with the germs which produce the disease known as chicken-cholera. It was thus:—

Pasteur found that by merely allowing the germs adhering to a finely-drawn-out end of a glass rod, which had received them by being dipped in chicken broth containing chicken-choleraic germs, to come in contact with perfectly clear, pure, and wholesome chicken-broth, the germs multiplied so rapidly in it that within a few hours silky cloud-waves of germs could be seen floating in it, and within twenty-four hours the whole had assumed a turbid appearance in consequence of the multitudes of germs it contained. At the same time the broth had lost its benign, and acquired fatal properties. Thus, for example: before it had been inoculated with germs, one, two, ten, even twenty cubic centimetres of it could be injected under the skin of a healthy fowl without inducing either disease or death; whereas, in twenty-four hours after its inoculation with the infinitesimally small quantity of minute germs, it had become so virulent by the multiplication of them in the liquid, that the injection of the mere fractional portion of a drop of it under the skin of an equally healthy fowl would not only produce its death, but transform its blood and tissues into a focus of cholera-germ poison.

Added to this, I may cite three different cases in which, after the deaths of the patients, I was an eye-witness of the astounding rapidity with which germs may be generated. In one of the cases, so rapid were the effects of decomposition from germ-development that their progress was actually visible to the eyes of the bystanders. The first case I shall refer to was one of death from a poisonous dose of cyanide of potassium—a dead toxic agent. The second was one of death from putrid blood infection—a living animal poison. The third, a case of sudden death from heart-disease in a patient of a rotten constitution. In all these three cases there was a most unusually rapid development of germs, and, as a consequence, a most extraordinarily speedy putrefaction of the tissues, due partly to the agents introduced into the system, and partly owing to the state of the weather. The details of the cases are briefly as follows:—

First Case:—Notwithstanding my familiarity with the fact that in all cases of death from hydrocyanic acid, or any of its salts, decomposition of the body takes place with unwonted rapidity, I was astonished to find that the body of what had been a beautiful, healthy, blonde girl of eighteen years of age thirty-six hours before, was, when I saw her, a green, blotched, and distorted-looking corpse, such as one sees in the dissecting-room six weeks or so after death.

The second case was no less remarkable. It was that of a girl aged about thirteen, if I remember right, who died after a few hours' illness, in a fulsome "Blood manufactory" for sugar-refining purposes in the East-end, regarding the cause of whose death I was requested to report by Mr. (now Sir) John Humphreys, the coroner. She, too, I saw within thirty-six hours of her death, and like the case above described, she was a green, putrefying corpse, smelling most offensively—like a dog that died after I had injected into his veins an ounce of putrid filtered water from a dissecting room macerating trough. The girl's death I attributed to infection received from one of the blood-preparing vats, which I verily believe—although the father of the girl and proprietor of the manufactory stoutly denied it—had contained diseased horse blood, from the knacker's yard. The whole establishment, notwithstanding that it had been furbished up with lime whitewashing in anticipation of my visit, smelt so abominably that I had to inspect it with my handkerchief over my nose.

The third case is the one which appears to me, as I have no doubt it will do to the reader, the most extraordinary of all. The cause of the rapid development of the germs seemed to be the sultry state of the weather, more than anything else, on a predisposed constitution. On this occasion I was present along with Dr. Priestley at the autopsy of a fat, wealthy, old gentleman of from sixty-six to seventy years of age, living in a fine house surrounded by fine pleasure grounds in the outskirts of London. Where the sanitary arrangements, I imagine, were all that could be desired. Yet, notwithstanding all this, and that life had only fled the body within two days, the corpse of this man was one mass of corruption. A human body exposed to the air for a month, even in summer time, could not have presented a more corrupted appearance. The skin was of a dark yellowish-green hue, covered with a layer of slimy moisture; the abdomen enormously distended; the face bloated out of human expression. In fact, I never in my life saw anything approaching to it except on one occasion in the Morgue at Paris, when I saw the distorted carcase of a human being which had been fished out of the Seine after decomposition had well set in.

Now comes the most extraordinary feature in this case. The smell of the corpse, notwithstanding two windows in the room being full open, and Condy's Fluid all around, was simply diabolical. But that was even nothing to the strange sight my eyes beheld in the form of human tissue decomposition made visible. As I stood by—as near as my olfactory nerves would permit, which I should think would be at about from four to six feet distance—and watched the surgeon performing his unenviable office, I saw globules of gas *actually forming* under the cuticle of the sides of the chest and abdomen—the body being open—run together into bullæ, which coursed along like living things under the epidermis. Sometimes by rapid starts, sometimes slowly. Uuite together into great blebs, which ultimately burst, and discharged their noxious gaseous contents. This startling fact of the visible formation of gas has an important bearing, as will be subsequently seen, on the germ theory. For the very essence of germ growth and multiplication, as I shall afterwards demonstrate, is its association with the evolution of gas, coupled with the disengagement of heat.

I ought not to leave this point without putting one more important fact on record, and that is, that germ development may continue uninterruptedly, after death from germ disease, for many hours, and keep up the bodily temperature above the normal standard. I observed this in the case of a well-nourished plump servant girl of one-and-twenty, whose body, after fifteen hours' sojourn in the cold post-mortem room of University College Hospital, in the winter time, felt quite hot to the touch, certainly feeling to my hand at least of a temperature of 102°. Unfortunately, having no thermometer in my pocket, I neglected to send for one and take the temperature. However, I have no doubt, from being experienced in manual temperature-taking, 102° is not above the mark. On the thorax being opened it steamed with heat, and on putting my hand to the heart it felt quite hot.

Now comes the question, What was the cause of this body retaining its abnormally high temperature for fifteen hours after the death of the patient? I believe, germ development. The girl was well nourished, and full, no doubt, of germ pabulum, for she had succumbed rapidly to a virulent attack of scarlet fever with violent cerebral symptoms; and from the pabulum in her system not being exhausted at the time of her death, the growth and multiplication of the scarlet fever germs continued just as they had done, though perhaps to a lesser extent than before death.

My views of the *rationale* of germ action in tissue and fluid decomposition will be more fully given in a book I am now engaged upon, in a chapter on the *modus operandi* of pathogenic germs. So, having shown with what immense rapidity germs may develop under favourable circumstances in the human body, both before and after death, I may leave the subject for the present.

From the foregoing data it is seen that the germ theory satisfactorily accounts not alone for the normal, but even for the abnormal, periods of incubation in the Infectious, Contagious, and Inoculable groups of disease. It behoves me, therefore, to endeavour in the next chapter to make it yield an equally satisfactory explanation of the Periodicity of Disease.—*Medical Times and Gazette, Nov.* 19, 1881, *p.* 596.

6.—ON DIAPHORESIS IN THE TREATMENT OF BLOOD-DISEASES.

By Charles F. Naismith, L.R.C.P. Edin.

There are 2,381,248 sweat-glands, more or less, in the human body. The average quantity of watery vapour excreted in twenty-four hours is two and a half pounds, one-third by the lungs. If this takes place naturally, how much more might be got rid of artificially? The chief function of the sweat is the removal of excess of water, with the dissolved products of animal combustion contained therein. The sweat, like the urine, is easily affected by drugs taken by the mouth, as shown by the administration of turpentine internally, &c. Why, then, should it not be used to throw off poisons which may have got into the blood by absorption through the alimentary canal, lungs, or as a secondary affection?

The skin performs the functions of the kidney, and excretes urea, when the latter organ is disabled. Why, then, should it not be made to increase its own activity where blood-poisoning has occurred? In pyæmia, does not the poison form abscesses all over the body, seeking an outlet and finding none? Why, then, do we not assist it to a means of exit, *i.e.*, the sweat-glands?

In fevers, do we not make use of diuretics and purgatives to stir up those excretories, the kidneys and bowels? But how many of us attempt to stir up the 2,381,248 sudoriparous glands? In the burning heat of fever, does not the production of perspiration cause an equable temperature to the whole body, and do we not view its occurrence in a fever-stricken patient with satisfaction? Yet how many of us attempt to produce it artificially, to rouse up the skin as we do the kidneys and bowels? The treatment of fever by cold applications has descended from Hippocrates, and no one but a fever-patient can appreciate the luxury of it; but there is a far more efficient agent than cold at our disposal, *i.e.*, the vapour bath. The wet sheet packing, or cold in any form, certainly reduces the temperature for the time being; but the vapour bath opens the millions of little apertures on the surface of the body, rousing them up to the fullest exercise of their excretory functions, getting rid not only of effete products of combustion, but other septic influences which may have entered the system, and, in addition to this great antiseptic performance, it lowers the temperature of the body quite as effectually as the application of cold could do. That the sweat does regulate the temperature in a very marked degree is obvious from the fact that it (the sweat) is the great safety-valve of Europeans in the tropics. A well-known writer of the present day has the following:—"The whole subject of body heat, its production, its dispersion, and their disturbances, call for more general attention than they have yet succeeded in attracting;" and he advocates the use of cold or tepid baths.

Jaborandi has been spoken of to produce diaphoresis; but why look to drugs when we have the means at hand in any bedroom? A cane bottom chair, blanket, tin pan, and spirit-lamp, and the requisite apparatus is before you. If the patient is unable to sit up, use Sir J. Y. Simpson's method in bed. There is no need to use the vapour bath to excess, any more than there is to give drugs in excessive doses, discretion being as necessary in the one case as the other. Let us not point at the British Pharmacopœia, saying, What! shall we use such a simple means as diaphoresis, when we possess this mighty armamentarium of drugs? Let us rather call to remembrance the story of Naaman the Syrian and the waters of the Jordan. —*Edinburgh Medical Journal, Dec.* 1881, *p.* 525.

7.—ON CANCER AND ITS COMPLETE EXTIRPATION.

By the Editor of the Lancet.

We have more than once had occasion to insist upon the importance of the localistic view of cancer as an encouragement to the surgeon in his endeavour to cope with this dread disease, and opportunities are continually being afforded in the advances made by surgery for a reiteration of this doctrine. Pathology has enabled us to define with fair accuracy the meaning of the term "malignant." It has taught us that there are some forms of new growth, which from their position or their own disintegrating changes imperil life, yet do not infect the system; they remain localised in the vicinity of their primary seat, and if capable of complete extirpation do not recur. There are others which infect neighbouring lymphatic glands, and seem to spend all their virus on them, seldom spreading to other regions. Others, again, with pronounced malignancy, are more or less rapidly disseminated throughout the organism. We believe that all the facts regarding malignant growths may be explained on the hypothesis of their local origin, as was some years ago so strenuously insisted upon by Campbell de Morgan; and the differences in degree and extent of generalisation depend as much upon the nature of the primary growth as they do upon the individual organism on which it is grafted.

Taking, then, this doctrine as established, how can we explain the lamentably frequent failure of surgery to radically cure the disease? To this question pathology supplies an answer, borne out by clinical experience. Take the instance of cancer of the breast. Excision is practised; a clean sweep is made of all the implicated structures and glands, as well as some of the apparently healthy surrounding tissue. One of several events may happen. No recurrence may take place, and a radical cure may be effected; but this is only too infrequent. In many cases in which it does take place, the new growth may be found to be of a simple non-infective character. Or there may be no local recurrence of disease; but sooner or later the lymphatic glands, disregarded at the time of the primary operation, become enlarged and cancerous, and may then be either removed or be so situated as to defy removal; whilst also internal viscera or the opposite breast may be the seat of secondary growths, and death ensue from such extension within a year or two of the first extirpation. The reason why this happens is that the disease has *not* been extirpated wholly, and this through no lack of judgment or boldness on the surgeon's part. Tissues that appear healthy, even to the microscope, may conceivably be already under the mysterious influence of the *materies morbi* generated by the growth of the primary disease; and if this may seem too extravagant a statement, it is certainly true of tissues that appear normal to the unaided eye. In this way only can the facts of local recurrence and recurrence in the lymphatic glands be explained. The same applies to the visceral metastases. At the time of operating no indication at all may be given of the liver being infected, and yet how many cases of mammary cancer eventually present extensive secondary growths in this organ. How can this be explained, except on the view that, even at the time when it appeared possible, and indeed in which it really was possible, to remove the primary disease, the liver was already infected, although a long interval — in some cases measured by years rather than months—might elapse before the latent germs manifest themselves in that organ. There is no need to apply the argument to all the other seats of secondary cancer, but it is not difficult to make the application, which seems to us to be a sound deduction from the pathology of these growths.

This idea does not, of course, preclude the notion that in many individuals there may be an underlying tendency to cancer, whereby they are more liable to its excitation from comparatively trivial causes than others are: a tendency which may perhaps be explained by a local perversion of nutrition or a congenital defect of certain tissues transmissible from parent to offspring. It does not seek to explain away "constitutionalism," but aims solely at emphasising the relation between the primary source of the disease and its secondary manifestations. This, then, is the encouragement to be derived by surgery from pathology—viz., to extirpate the local disease at the very earliest moment, so as to anticipate its generalisation; to remove as much tissue as possible within the region of the primary disease, going far beyond the limits of visibly altered tissue. There is nothing new in either of these principles, which are daily enforced by practical surgeons, and yet the ultimate issue is too often the same.

There have been two papers read before our Societies this week that bear on this subject, and have, indeed, suggested the foregoing remarks. At the Royal Medical and Chirurgical Society Mr. Bryant communicated a case in which he had successfully excised a cancerous stricture of the colon through an incision in the loin, made with the object of colotomy, symptoms of chronic obstruction having lasted a long time, and practically complete obstruction for eight weeks. The diagnosis of annular stricture in the descending colon or sigmoid was made, and confirmed when the bowel was exposed. Finding that the gut had contracted no adhesions to the surrounding parts, and knowing the very local character of these cancers of the bowel, Mr. Bryant relieved the patient of the diseased mass, and has probably succeeded in effecting a complete cure. Colotomy alone would have done much: the obstruction would have been relieved, and the ulcerating growth, no longer irritated mechanically, would have proceeded at so slow a rate as to have given but little trouble from its local extension for a long time; but its presence will ultimately probably cause the patient's death from implication of the surrounding parts in the disease. Colotomy and complete extirpation of the ring of diseased bowel has done more—in fact, has left nothing more to be done. The other paper was read by Mr. Pearce Gould before the Medical Society, and in it the principle was advocated not only of free extirpation of the diseased tissues in mammary cancer and of the implicated axillary glands, but also of those glands which, to all appearance, are healthy, but, from their close contiguity to the infected glands, may be on the verge of becoming themselves infected, and possibly are so. He quoted some striking illustrations of this, and we cannot but think that the procedure he advocated is based on rational pathological grounds. The logical outcome must not, however, be disregarded. It means this, if it means anything, that in no case is it wise to leave behind glands which can be extirpated, although they may present no evident signs of disease. It applies to cancers of the lip, tongue, penis, and other regions where the surgeon is enabled to remove the primary disease, but where glandular recurrences are so prone to occur, just as much as to the mamma. We say nothing of the practicability of such radical interference, and we are not unmindful of the fact that such glandular recurrence is not invariable, but we can see no hope for the complete extirpation of cancer—none, in other words, for its cure—until the stern pathological fact of glandular infection is recognised in all its significance by surgery.—*Lancet, April* 1, 1882, *p.* 531.

8.—ON CANCER, SARCOMA, AND LOCAL INJURY.

By the Editor of the British Medical Journal.

The relation of local violence to malignant disease is an unsolved question of great importance. No modern authority doubts that the origin of epithelioma is very frequently due to local irritation of the most tangible kind. Every student learns, in his text-books, about cancer of the lip and tongue which may follow the irritation of a pipe; about cancer of an old neglected ulcer of the leg, irritated by dirt and friction; and about cancer once common on the scrotum in chimney-sweeps. The irritation which certain obscure stimuli set up in the mammary gland causes, according to Dr. Creighton and his followers, an imperfect process of secretion, with formation of imperfectly developed cells, very prejudicial to the surrounding tissues; indeed, these cells appear to be the starting point of malignant disease. All these processes by which local epitheliomata and mammary tumours develop are chronic, and are not the result of what an Englishman understands by the word violence. In association, however, with this part of the question, let it be remembered that irritation means chronic injury, violence, acute injury. Irritation may produce well known forms of inflammation; it also causes malignant disease. Violence sets up equally well-known types of the inflammatory process; and Mr. Barwell, at page 187 of this number of the British Medical Journal, brings forward three instances, all well authenticated, of sarcomata following severe blows or fractures. Two were in his own practice, one was under the care of Sir James Paget. We know of a fourth case, where a large subcutaneous extravasation of blood followed a blow on the shoulder. The fluid part of the blood was removed by an aspirator. Within a few weeks, a sarcoma developed in the shoulder, and rapidly recurred after removal. In fact, the case precisely resembled one described by Mr. Barwell. That gentleman shows, in the paper to which we have referred, that the relation of the violence to the growth of the tumour cannot be mere coincidence. It is absurd to suppose that, in his case of a youth who died after recurrence of a sarcoma which appeared on the shoulder a few weeks after receiving a blow there, malignant disease happened to be on the point of development when the injury occurred; and, if such an extraordinary coincidence could explain this case, it is otherwise with the second related by Mr. Barwell. A stevedore bruised his left side in a fall down a ship's hold; within a few weeks he died of that comparatively rare disease, sarcoma of the lung and pleura, the left lung being the most involved. Mr. Barwell terms this condition acute traumatic malignancy.

Inflammation is, in these days, usually defined as a series of changes occurring in a part subjected to injury not sufficient to immediately and completely destroy its vitality. On the other hand, this "acute traumatic malignancy" is observed under the same circumstances as those included as part of the definition of inflammation. The ancients placed cancer among inflammations; many of the physicians and surgeons of antiquity were profound clinical observers, and must have noted some relation between malignant growths and injuries. On the other hand, as we have just indicated, there is a distinct relation between violence and irritation. The latter sets up inflammation, and also may produce malignant disease. The former decidedly causes inflammation, and more rarely, but equally surely, according to the above evidence, it may set up malignant disease as well; others, long before Mr. Barwell, whose valuable clinical records are most welcome, have recognised this fact. According to these data, inflammation and malignant disease would appear to be, very possibly, conditions actually different in every pathological sense, but liable to be produced by the same causes. It seems more probable, however, that this coincidence points to some very intimate relation of the two morbid conditions as yet unknown. In any case, acute traumatic malignancy includes an integral part of the definition of inflammation.—*British Medical Journal, Feb.* 11, 1882, *p.* 197.

9.—ON THE USE OF CHIAN TURPENTINE IN CANCER.

By Professor John Clay, Obstetric Surgeon to the Queen's Hospital, Birmingham.

More than two years have elapsed since I commenced treating cancer of the female generative organs with Chian turpentine. By the courtesy of the conductors of The Lancet the results of my first experiments were published in the number for March 27th, 1880. The paper attracted considerable attention, both professional and general, and provoked much adverse criticism; indeed, in certain quarters this plan of treating cancer of the uterus in particular was declared useless. An enlarged experience, however, has confirmed the statements made in my original paper, and I have now the satisfaction of being able to declare that I have nothing to withdraw or to qualify as regards the statements I then made, as the result of observation, as to the effects of Chian turpentine in uterine cancer. I should be glad to confirm these conclusions by now publishing a number of illustrative cases showing the treatment pursued in different forms of uterine cancer; but consideration of the space at my disposal obliges me to be content with describing briefly the conditions under which a measure of success may be obtained, and the opprobrium of the alleged uselessness of the remedy may thus be removed, to the ultimate benefit of suffering humanity. It appears therefore necessary to determine whether Chian turpentine does actually alleviate the distress of cancer, or has any controlling influence upon the progress of the disease, or any pretensions to effect its cure. It is obvious that if either or all of these results can be secured by the use of the drug, in any form or situation of cancer, the remedy cannot be deemed useless. Now, the facts within my own knowledge, derived from my own professional experience, may be summed up as follows. Nine cases of cancer confined to the uterus, which have been under treatment for about twelve months, are so far convalescent that they are no longer under observation. The cancerous growths have disappeared, there is no bleeding on manipulation, and the parts are smooth to the touch, and appear to be covered with mucous membrane. In most of the cases the cervix uteri is shortened from the contraction consequent on the removal of the growth. A number of cases of uterine cancer in private and hospital practice are under treatment in which freedom from pain, diminution of hemorrhage, and sloughing of the growth, with improvement of the general health, are prominent features. A number of advanced cases of uterine cancer have been treated for a short time, and the patients have died, but an amelioration of the more severe symptoms took place, although the patients succumbed to the anæmia produced by the previous exhausting discharges. The disappearance of the cancerous growth was verified in three cases where an examination was made after death. One case was complicated with cystic disease of each ovary, one died from dysentery, and in the other the glands in the pelvis and abdomen were extensively diseased. In neither of the fatal cases where the remedy had been exhibited for some time were there any fistulous communications with the rectum or bladder. If we bear in mind the progressive and generally rapid advance of the disease when left alone—as it rarely disappears spontaneously—or when it is treated by palliatives, and compare this with the treatment by Chian turpentine, we find in the latter method a gradual subsidence of the disease, varying as to length of time in proportion to the more solid consistence of the growth, a marked diminution of pain, a lessening of the hemorrhage, with an increase of the muco-purulent discharge. There is usually an improvement in the general health, but an increased tendency to anæmia is sometimes noticed. From a review of all the cases I have observed, it may be safely asserted that the effects of Chian turpentine in cancer of the uterus are tolerably uniform, and it is more than probable that the remedy effectually removes the cancerous infiltration surrounding the original growth, thereby preventing the extension of the disease. In some instances the treatment has removed glandular complications, and in others it is presumable that these have been averted. It is this controlling action of the drug which probably causes the abatement of the pain and hemorrhage. The growth gradually diminishes in size, becoming loose and shrivelled, and losing its firm and succulent condition. Whatever may be the termination of the case, the treatment manifestly affords to the patient such comfort as is not obtainable by any other therapeutical measure.

The earlier the cancerous disease comes under treatment, the greater is the prospect of ultimate relief. The success obtained in recent cases is probably owing to the patients coming under observation before any apparent extension of the disease, or before vital organs are involved. Where the vagina is affected with the disease primarily or secondarily, particularly the latter, the prospects of relief are materially reduced. The treatment should be prosecuted vigorously and persistently, especially at the outset, so as to minimise the constitutional effects of the disease, as it is difficult to decide in what stage these are developed, and perseverance with the treatment should be strenuously encouraged by the medical attendant, as otherwise the patient may be left to die unaided by the only drug that has been found by its internal administration to have any pretensions to resolve a cancerous growth.

In treating a case of cancer of the uterus or rectum, the following procedure is recommended for adoption. It may be premised that it is essential that the genuine drug only should be administered. It is a humiliating statement to make that even now some houses are supplying and dispensing other turpentines for Chian turpentine, and are even guaranteeing the genuineness of the article they supply. It is also necessary that the drug should be given in the form which is most convenient for assimilation. The essence of Chian turpentine, prepared by Messrs. Southall and Barclay, of Birmingham, appears to me to be the most suitable preparation. This is prepared without the sulphur, and evidently does not contain any ether. The drug is in a state of minute subdivision, is easily digestible, suitable for all forms and situations of cancer, and is very palatable. One teaspoonful of the essence contains three grains of the turpentine. In place of the sulphur in the essence, Messrs. Southall prepare pills which contain sulphur, sulphate of copper, &c., and which they style, "pil. sulph. comp.," to distinguish them from the Chian turpentine pills. Two teaspoonfuls of the essence, with one or two of the compound sulphur pills, should be given three or four times a day, and after the medicine has been taken for about three months it should be omitted for about three days in every fortnight. The pills or mixture, prepared according to the original formula, may be given instead of the essence and sulphur pills.

The vagina and rectum, even from the first, should be syringed daily with equal parts of vinegar and water. After allowing time for this to drain from the parts, it is advisable to insufflate into the vagina or rectum about ten grains of the following powder:—Tannic acid half an ounce, powdered charcoal two drachms, and powdered sulphate of copper ten grains. Messrs. Mappin and Co., of New Street, Birmingham, make a vaginal insufflator by which the powder may be introduced into the rectum or vagina very efficiently. An insufflator may be improvised out of a vulcanite tube and a two-ounce india-rubber bottle connected with elastic tubing, but such an instrument is apt to become moistened by the secretions, and thus prevent the powder from becoming properly placed. The propriety of excising the os uteri in epithelioma of this part, as a preliminary procedure to the use of the Chian turpentine, may be questioned on reasonable grounds. The cancerous growth as it disappears under the Chian turpentine treatment leaves a shortening of the os uteri which brings the external rim of the os uteri into close proximity with the bladder and rectum, so that if the vaginal portion of the uterus has been removed, the subsequent contraction of the lower portion of the uterus drags on the rectum and bladder, causing great pain, with rectal and especially vesical troubles. In large pedunculated epithelioma of the os uteri the larger portion of it is perhaps best removed, taking care to leave the normal uterine tissue intact. In cases where the turpentine has been taken for some months, the dull curette may be used to remove the sloughing mass with advantage. When pain in the sacral or hypogastric regions comes on after the treatment has been pursued for two or three months, the use of morphia suppositories is indicated. To avoid the habitual use of opium, the tincture of Jamaica dogwood (Christy's), in one-drachm doses once or twice during the evening may be prescribed. Hemorrhage at the monthly periods is best met by giving the liquid extract of ergot in ten- or fifteen-minim doses (which may be added to the essence of Chian turpentine), and to use locally the perchloride of iron in solution, or the dried persulphate of iron by means of insufflation. Diarrhœa and dysentery sometimes supervene during the treatment, for which the oil of eucalyptus globulus (in five-minim doses three times daily for a few days only) is a good remedy. This drug may be added to the essence of Chian turpentine. Anæmia consequent upon the occasional hemorrhages and serous discharges is a serious complication; much of the local treatment here recommended has been suggested to anticipate or prevent the anæmia. To combat this condition, Fellows' syrup of the hypophosphites, in one-drachm doses, may be given during meals, from the commencement of the treatment, in connexion with the Chian turpentine treatment.

In cancer of the vulva Chian turpentine acts slowly, and as the disease frequently extends rapidly, an early excision of the growth, if possible, is necessary, the remedy being given for some time afterwards with a view to prevent a recurrence of the disease.

The use of Chian turpentine in a large number of cases of cancer of the breast shows that it is a remedy of considerable power in relieving pain, of diminishing the size of the growth by causing the removal of the cancerous infiltration, leaving the more permanent fibrous stroma to be subsequently dealt with. In many instances the growth, although at first it was firm, and seemingly adherent to the chest walls, has become loose and easily movable from side to side, as if it were merely a foreign body, so that if an incision had been made in the skin it could have been pushed out. Several cases of primary scirrhus of the breast have been noticed in which, after the administration of the Chian turpentine for some months, the whole of the cancerous mass has sloughed away, appearing as a black gangrenous mass, leaving a cavity of considerable dimensions, the walls of which were composed of healthy tissue, and the wound healed with comparative rapidity by granulation. This process, however, is a slow and painful one, and suggests the propriety of anticipating the sloughing process by removing the growth with the knife after it has become sufficiently detached from the surrounding structures by the treatment. Several cases treated by this method have done well, the operation has been materially simplified, and, if the remedy is administered for some time after the operation, the experience gained shows that there is not so much likelihood of a recurrence of the disease. In recurrent cancer of the breast the remedy is often found to be of great benefit, and after it has been given for some weeks the application of the crystals of resorcin to the growth facilitates the disintegration of the mass. Resorcin is a powerful and at first a painless application. When its use is attended by much pain it may be mixed with equal portions of tannic acid and charcoal with good effect. Vaseline or chrisma dressing is all that is required after the application of the powder.

In epithelioma of the face and other parts of the cutaneous surface the use of Chian turpentine in conjunction with the powder previously advised (when stating the local treatment of cancer of the uterus), to which three drachms of resorcin have been added, very good results have been obtained. In two cases of cancer of the stomach very beneficial results have accrued from the special treatment. In cancer of the mouth and tongue the results have not been so good, in consequence of the rapidity with which the neighbouring glands became involved in the cancerous disease.

I leave these necessarily incomplete observations to the impartial critical judgment of the profession. Enough, I trust, has been stated to show that the Chian turpentine treatment has some ameliorating influence on cancerous disease — how much the future must determine—and therefore I am enabled to claim that the remedy is far from being useless. The facts stated may be considered at least as a distinct addition to our existing stock of knowledge in the treatment of cancer, and I look forward as the almost certain result to their being confirmed, and to candid inquiry leading to further improvements in the same direction.—*Lancet, Dec.* 17, 1881, *p.* 1033.

10.—CHIAN TURPENTINE IN THE TREATMENT OF CANCER.

By Dr. John Binnie.

A patient of mine had cancer of the mouth, in regard to the diagnosis of which there existed not the least doubt. I procured for her a pound of the emulsion of Chian turpentine, which she commenced taking on the 1st January last. This finished, she procured more of the same, and has now consumed three pounds. When she commenced this treatment the fœtor was intolerable, the pain very severe, and the disease making rapid and steady progress. In a few days the fœtor was very much diminished, and improvement in this regard continued until the smell almost entirely disappeared. The pain also diminished very materially, and for a time the swelling diminished and the appetite improved, raising the hopes of both patient and friends. After a time, however, dislike for the emulsion supervened, and gastric disturbance followed each dose. The emulsion was, under these circumstances, necessarily suspended; and although the pain and fœtor have returned, it is with considerably diminished severity. The general condition of the patient leaves little ground for the hope of her recovery; but this fact does not negative the other fact that Chian turpentine exerted a favourable influence over the course of the disease, and that it is as good a remedy, at least, in this disease as any heretofore tried; in my experience it is a better. I certainly had never seen a medicine before have such a direct influence over the morbid process.—*Edinburgh Medical Journal, Dec.* 1881, *p.* 555.

11.—ON THE ADVISABILITY OF ENUCLEATING THE AXILLARY GLANDS IN CASES OF REMOVAL OF SCIRRHOUS MAMMÆ.

By A. Pearce Gould, M.S., Assistant-Surgeon to the Westminster Hospital.

In all cases where the mamma is amputated for cancerous disease the surgeon should remove also the axillary lymphatic glands. It is a cardinal rule in surgery, and very rightly so, that excisions of cancerous growths should only be undertaken when the surgeon can remove the whole of the diseased structures.

It will be convenient to divide the cases of cancer of the breast suitable for operation into: (1) Those complicated with evident enlargement of axillary lymphatic glands, and (2) those not so complicated.

1. *Is it right or is it wrong to operate for cancer of the breast where the lymphatic glands in the axilla are obviously enlarged from secondary cancerous growth?* The answer to this question

depends upon the answers to two others: Can we safely remove cancerous glands from the axilla? and, Does the cancerous infection stop at the lymphatic glands for so long a period that there is time for those glands to become distinctly enlarged before the malignant disease has been carried to, and grown in, internal organs? The practicability of the operation must of necessity vary with individual cases. Where the glands are still freely movable it is easy to enucleate them; where they are firmly fixed down to the chest wall, it is impossible to remove the whole of the diseased structures without serious risk, or even the certainty, of fatal injury to the thoracic cavity. Between these two extremes there are several grades of severity. But if we look at the dangers and difficulties of the operation, we shall best arrive at the true answer. By drawing the arm well away from the side, and prolonging the mammary incision outwards, the axilla is easily exposed. The dangers are the primary ones of wound of the large blood-vessels and nerves, and wound of the pleural cavity. As the blood-vessels and nerves are very constant in their position the surgeon always knows where his danger lies, and even when the growths are adherent to these parts he can often, by slow and very careful dissection, separate them without wounding either vessels or nerves. The intercosto-humeral nerve, which crosses the axillary space, is most of all exposed to injury, and must be very generally cut or torn across. One common way of avoiding the danger of hemorrhage is to separate the mass to be removed, except at the end next the vessels, and then to throw a strong ligature around this pedicle, and cut the mass away on the distal side of the ligature. This is only a "surgical makeshift." It is far better, by care and patience, especially patience, to separate the growth all round. If vessels of any size are found entering it, ligature them separately. The disadvantages of the makeshift plan are, that the vessels in the pedicle are not so securely tied; nerves as well as vessels may be included in it; and there is no certainty that the surgeon has removed all the diseased tissues.

The secondary dangers of the operation are suppuration in the axilla and thrombosis of axillary vein. In my own experience these accidents have not occurred, and I have found these wounds in the axilla heal very well, if I took three precautions: to put a drain up into the cavity, to apply suitable padding to keep the walls of the cavity in apposition, and to keep the arm fixed to the side.

I may briefly relate one case of removal of cancerous axillary glands under my own care.

On Aug. 30th, 1880, I operated at Westminster Hospital on C. C——, aged fifty-one, who had a scirrhous tumour in the right breast as large as an orange, and enlarged glands in the axilla the size of an apricot. After removing the mamma and a large portion of the skin over it in the usual manner, I carried the incision up and out into the axilla, and first of all carefully detached the cancerous glands from the side of the chest; it was then found that they were not only adherent to but growing completely round the axillary vein and some of the nerves of the brachial plexus. I cut down through the mass on to the coat of the vein, and was then able to peel the glands off the vein and nerves, leaving them uninjured. Unfortunately, before doing this I had put a ligature on the vein, as it at first seemed impossible to finish the operation without wounding it; this precaution, however, proved to have been unnecessary. The external mammary and one small superficial artery were the only vessels ligatured. The axillary wound healed at once, except a narrow track occasioned by the silk ligature round the vein. I saw this woman last year. She was then well. I need not quote other cases I have had; this was perhaps the worst of all. The only time I had any hemorrhage was once when I wounded the subscapular vein close to its junction with the axillary; it was easily secured with a ligature, and caused no further trouble.

As to the second question, I think all will agree that there is often an interval of many months between the discovery of cancerous infection of the axillary glands and the appearance of signs of cancerous disease in the internal organs. But the *facts* I would adduce in support of this statement are these:—(1) That systemic secondary cancer without antecedent glandular infection is very rare, being met with in only about 13 per cent. of such cases. (2) That in cases of death from cancer of the breast, infection of the glands is met with far more frequently than infection of any other organ. Thus, in 128 autopsies, the glands were found cancerous in 115 cases, and the liver, which stands next in order of frequency, was infected only fifty-five times; the lungs twenty-eight times; and then comes a great fall to units. What is the meaning of these facts but that the path of the cancerous poison from the breast is through the axillary lymphatic glands, and that the poison is delayed in its passage through these glands, often for a long period? Hence, on pathological grounds, this operation is justifiable. Practically, it is a safe procedure, not adding greatly to the risk of the simpler operation, and clinically it is successful; for, out of ninety-three cases tabulated by Oldekop and Winiwarter, in which this was done, the mean subsequent life was 39·3 months. And Gross states that in as many as one-third of the cases submitted to a complete operation—*i.e.*, clearing out the axilla—no recurrence was observed up to so long as six years afterwards.

2. *Is it right, or is it wrong, when operating for cancer of the breast, in which no lymphatic enlargement can be detected, to remove the axillary lymphatic glands?* Practically this is a very simple procedure. A very small extension of the wound allows the finger to be passed into the axillary space, and with it the glands can be readily enucleated, without danger to nerves, vessels, or pleura. Or, better, a careful dissection of the axilla can be made. The small twigs of alar thoracic vessels, which are necessarily divided, are not large enough to require any treatment. A small drain should then be put in the axilla and the wound treated as already mentioned. Such a procedure hardly, if at all, adds to the danger of the original operation. But is it of any advantage? I believe it is, for I believe that in the very earliest stage of lymphatic infection it is impossible to detect any enlargement of the glands by the ordinary examination through the skin. Such a case occurred to me quite lately. A patient was brought to me by a friend, Dr. Shaw, of Enfield, with a scirrhous cancer in the right mamma. Neither I nor Dr. Shaw nor Mr. Macnamara, who afterwards saw the case with me, could detect any lymphatic enlargement. But after excising the mamma I, as a matter of routine, proceeded to remove the axillary glands, and then I found that two of them were evidently already cancerous, in one the focus of disease being about the size of a shot, in the other of a pea. My friend, Dr. Heneage Gibbes, has prepared some microscopical sections of those glands, which prove the cancerous nature of the growths. Now here was convincing proof that had I simply contented myself with excising the mamma, I should almost directly have found a recurrence in the axilla, and this one case was sufficient to convince me that cancer may exist in axillary lymphatic glands which cannot be detected by the ordinary methods of examination. The only other explanation is, that had I used proper care and skill I should have detected these minute growths. To which I can only reply, that I examined the axilla repeatedly and with care, and that if I made an oversight, so did Dr. Shaw and Mr. Macnamara, and therefore it is an oversight that may be expected to be made. Further, recurrence in the axillary glands, apart from local recurrence, is not uncommon after removal of the mamma alone. And recurrence in the axilla is far more frequent after removal of the breast alone than when the glands are simultaneously removed. The axillary glands are not of functional importance at the age when cancer occurs.

In conclusion I would formulate my argument thus:—

1. Partial excision of a cancer should never be performed.
2. Where breast and axillary glands are cancerous the breast alone should never be excised.

3. That in very many cases of cancerous axillary glands excision can be performed with only slight additional danger, and with great benefit to the patient.

4. That the cases where such excision is not practicable are those in which the growth has spread to the chest walls. Pressure upon, or even adhesion to, the main blood-vessels and nerves of the axilla is not an insuperable bar to the operation, if sufficient care be taken.

5. That when no enlargement of axillary glands is to be made out, those glands may yet be infected with cancer, and that, therefore—

6. Even when not evidently diseased, they should be removed at the same time as the diseased mamma, or, in other words, "it is advisable to remove the axillary glands *in all cases of cancer* of the breast submitted to operation."—*Lancet, April* 8, 1882, *p.* 560.

12.—ON THE TREATMENT OF RHEUMATISM.

By the Editor of the Medical Press and Circular.

Dr. Francis Warner concludes, from observations of 190 cases of rheumatism, acute and sub-acute, in all of which a temperature exceeding 102° F. was recorded, that "salicin lessens the duration of the pyrexia and joint pain." The average duration of pyrexia in these cases was five and a half days, while under treatment of 79 cases without salicin the average duration extended to 13·8 days. Similarly, pain persisted for 5·3 days under salicin in 277 cases, and for 9·2 days in 67 cases treated on other lines. Finally, the average length of confinement to bed of cases treated with salicin was 19·5 days in 342 instances, and in 211 without salicin it amounted to 23·5 days.

An important point, however, now remains to be considered, viz., in what degree are heart affections influenced under the two modes of treatment? and on it Dr. Warner's conclusions differ only slightly from those of previous observers. He records that in 13·9 per cent. of the patients submitted to salicin treatment there was development of heart symptoms, while these were exhibited in 14·9 per cent. of those otherwise treated.

Next we will take the statement of Dr. De Haviland Hall, whose conclusions are based on an examination of 55 cases of acute rheumatism in the wards at Westminster Hospital during 1880. Salicylate of soda was the drug employed in all those cases where this treatment was adopted, 38 in number; in 14, heart complications occurred, while in five of the remaining 17 similar symptoms were developed. Dr. De Haviland Hall expresses himself in favour of salicylate treatment, which he avers tends to reduction of time during which fever and pain persist; and in reference to the heart complication he distinctly admits the superiority possessed by it, on the ground of the more acute nature of the cases in which it is resorted to. Toxic influences ascribed to the salicylate do not, in his experience, weigh at all heavily; salicinism occurred in eight cases, but left no permanent effect, and the general tenour of his observations is distinctly favourable to the employment of the drug in acute cases.

Dr. Isambard Owen tabulates 210 cases of acute and subacute rheumatism treated in 1877 and 1878 in St. George's Hospital; the results pointed by these are markedly favourable to the salicylic treatment. Comparison of cases treated thus, with those in which alkaline remedies were adopted, shows that under the former plan the average duration of pyrexia and pain was 3½ to 4 days, while under the latter the average of pyrexia was 6½ days, and of pain 8 days. Dr. Owen arranges his salicylic cases in three tables accordingly as the initial amount of drug prescribed exceeded three drachms, was equivalent to 2 drachms, or did not exceed 1½ drachms per diem. The toxic effects produced were found to be in proportion to the size of the doses administered. Thus, in the first-class, they followed in 23½ per cent. of the cases; in the second-class, in 13¼ per cent.; and in the third-class, in 10¼ per cent.; and a hope is expressed that these embarrassing consequences may, in the future, be avoided—a not impossible event. The average days of illness, again, under each mode of treatment affords an interesting study. Where the larger quantities of salicylate were administered, it reached 6 days from the commencement of treatment. It was a similar time in the second-class, and 7 days in the third-class; while under the simple alkaline treatment it reached 10¼ days. Heart affections were observed in several cases, Præcardial friction was found in four cases treated with full doses of salicylate, and in one case treated with full doses of alkali; but Dr. Owen infers that in all but one case the condition preceded treatment. Albuminuria did not seem to be a consequence of the treatment in any of the cases. There was no occurrence of hyperpyrexia in any instance, and no death was recorded. Here, too, then, abundant evidence is afforded of the superiority of the new treatment over the older method, and testimony to the freedom from dangerous complications, of which suspicions have been freely and frequently expressed in the immediate past.

Dr. Donald Hood, however, of the West London Hospital, in a careful analysis of 1,200 cases of acute rheumatism occurring in persons under 35 years of age, arrives at conclusions, which, substantiated as they are, must be very carefully considered in their bearings on the question at issue. He also demonstrates that patients taking salicylate lose their pains more quickly than those who do not take the remedy. And this is further borne out by Dr. Hilton Fagge's results. Against this, however, must be placed the fact that patients so treated *relapse* with far greater frequency than when other forms of treatment are followed. They thus remain, in the aggregate, a longer time in the hospital, and, Dr. Hood asserts, with Dr. Fagge, are left feeble and exhausted after the use of the drug. Again, with respect to cardiac complications, Dr. Hood finds that the proportion of those affected under one or other form of treatment is much about the same; and concludes that, "What little advantage there is does not appear to be on the side of the salicylate treatment." Further, he insists that any remedy claiming to be of "specific" effect in acute rheumatism must be capable of controlling heart disease; and this power is manifestly wanting to salicin.

Dr. Hood's tables are very valuable; they are incontrovertible, but they are also disappointing. They practically illustrate the uncertainty that still surrounds the whole question, and they cannot be overlooked in the present aspect of the controversy. It may, of course, be said that he attaches undue importance to certain signs and appearances; but with the data he supplies to refer to, it will be difficult for his opponents to rebut his evidence, or to refute the unexpressed conclusion that contrary decisions to those he expresses are untenable. Out of 1,200 cases he has summarised, it is true that only 350 were subjected to the treatment now on its trial, while 850 were otherwise treated. Nevertheless, it remains that even this number is a higher total than any advocate of salicin has been enabled to produce; and the results exhibited in the tables suffer nothing by comparison with other statements. It is, undoubtedly, in the conclusions drawn by him that Dr. Hood's position is a strong one, and until they are met by contrary evidence they will continue to demand the careful attention of every practitioner.

The influence exerted by impurities in salicylic acid forms the subject of a short paper by Dr. J. K. Fowler, who ascribes to them the toxic effects following salicylic treatment; and suggests that employment of a pure drug, together with properly-arranged wards, maintained at a sufficient, equable temperature, would ensure a greater amount of success in the treatment of rheumatism by such means.

Dr. Sidney Coupland contributes an analysis of 84 cases admitted under his care into the Middlesex Hospital during two years. His conclusions are favourable to the use of salicylate of soda in their treatment, and he enters at length into a consideration of the various details.

The lesson taught by this discussion is a most useful and important one. There is a common agreement that certain definite effects are produced by salicin in cases of acute rheumatism, and that these effects may be fairly anticipated to follow from its employment. Whether, however, the advantages of speedy reduction of pain and fever are absolute, or are countervailed by equally weighty disadvantages, is the point on which as yet it is impossible to say a decision has been arrived at. We cannot conceal from ourselves that the advocates of salicin all seem determined to accept its good qualities, and to ignore its potentiality for evil; but with the evidence, and the deductions from evidence, afforded by Drs. Hilton Fagge and Dalton Wood, are we, it is fair to inquire, justified in regarding the matter as other than *sub judice*? Honestly, we think not; and honestly, too, we believe that further and more complete and extended investigation—especially of the complications set up by it—on the effects produced by salicin, are required to enable a reliable estimate of its character as a remedy to be formed.—*Medical Press and Circular, Jan.* 11, 1882, *p.* 29.

13.—ON THE TREATMENT OF ACUTE RHEUMATISM BY SALICIN AND THE SALICYLATES.

By T. J. Maclagan, M.D.

An intelligent appreciation of the mode in which the salicyl compounds arrest the rheumatic process, presupposes a knowledge of the nature and mode of production of that process. I have recently given to the profession my views of the pathology of rheumatism, and shall not enter on them now. I make this brief reference to the subject in order that the Society may understand what I have to say regarding the manner in which the salicyl compounds produce their anti-rheumatic effects.

Acute rheumatism I regard as a malarial fever; the rheumatic poison I regard as a malaria; and malaria I believe to consist of (and all recent research tends to confirm this view) minute organisms. The symptoms of acute rheumatism I regard as the result of the action of this special organism on the system. The local joint and heart affection are the result of the action of these organisms on the fibrous textures of the joints and heart. The salicyl compounds produce their anti-rheumatic effects not in virtue of any special action which they exercise on the system, but solely in virtue of their destructive action on this organism: they arrest the rheumatic process not by acting directly on the affected tissues, but by destroying the poison which causes disturbance in these tissues. During the course of this action the physiological effects of the salicyl compounds—that is, the evidences of their action on the system—are frequently manifested; but these effects, so far from being looked upon as essential, are observed with regret; we do not wish them, and are very sorry when they appear; but if it were their full action on the system that we wanted, we would give them till we did produce these effects, and would give them with that object. That we get the full curative effects of this drug in the majority of cases without its physiological effects, is evidence that it is on the rheumatic poison that they act, and not on the system.

This leads to the second point—their mode of administration. All evidence shows what, from the time that I first introduced this mode of treating acute rheumatism I have all along persistently maintained, that to get their full beneficial action the salicyl compounds must be given in full and frequently repeated doses. The view which I advocate as the mode of production of the rheumatic process, and as to the mode of action of the salicyl compounds, shows why this should be. The object in view is the destruction of the rheumatic poison. This object is attained by administering the salicyl compounds. The larger the quantity that can be thrown into the system in a given time, the more rapid will be the destruction of that poison. The salicyl compounds are very rapidly eliminated from the system, hence the necessity for giving them frequently as well as in full dose. What is wanted is the presence for some time in the blood of as much of this anti-rheumatic agency as can well be borne.

An important part of the question of the mode of administration of the salicyl compounds is, Which of these compounds is best borne by the system? Salicin and salicylic acid, be it borne in mind, are only two of a numerous series of salicyl compounds, all of which may be possessed of anti-rheumatic properties. From observations made with saligenin and salicylous acid I feel sure that they are anti-rheumatics. Practically we have to deal only with salicin and salicylic acid (generally given as salicylate of soda). Salicylate of soda is most used; but no one who has used it much can have failed to notice its deleterious effects on the system. Many instances have been recorded, and their number is being swelled in almost every journal that we read, in which more or less disastrous effects have been produced by it—delirium, insanity, prostration of the vital powers, syncope, and even death. Salicin is equally powerful as an anti-rheumatic, but it produces none of the deleterious effects of the salicylates. In several cases, some of which I have recorded, I have given full doses of salicin to patients suffering from the depressing and disturbing action of salicylate of soda; and under its use (though, of course not in consequence of it) the depressing effects of the salicylate have disappeared.

In the statistics which I have laid before the Society to-night it will be seen that salicin is used in eleven of the thirty-nine hospitals named; in two of the eleven it is used to the exclusion of the salicylates. While the salicylates are frequently said to produce bad effects, not a single reference is made to any bad effects following salicin. In most cases the deleterious effects of the salicylates pass off with the omission of the drug; but to have to omit it is a serious drawback, for its early omission is most likely to be followed by a renewal of the rheumatic symptoms.

Whether or not the salicyl treatment of acute rheumatism diminishes the frequency and danger of heart complications, is a point on which I have not time to enter. I believe that to some extent it does. But I believe also that when these occur the treatment by salicylate of soda occasionally increases the patient's danger. I refer specially to those cases, more numerous than is usually supposed, in which the muscular substance of the heart is the seat of inflammation. Myocarditis has for its pathological condition thickening, softening, and enfeeblement of the muscular walls, chiefly those of the left ventricle. If to this enfeeblement is added that which sometimes follows the administration of salicylate of soda, the patient's condition is thereby rendered more serious, and the continued administration of the salicylate might turn the scale against him.

One word on relapses. These are generally said to be more frequent under the salicyl, than any other form of treatment, though Dr. Owen's statistics would seem to indicate that this is more apparent than real, and may be a mere result of more attentive observation. There is no doubt that the salicyl treatment has to be kept up for some time after the symptoms have abated, and that their too early omission is apt to be followed by a return of the symptoms. But the term relapse as applied to acute rheumatism I believe to be pathologically erroneous. Dr. Southey has divided cases of rheumatic fever into the continued and the relapsing. But rheumatic fever never is a continued fever as that term is usually applied in medicine: it never goes through a continuous and regular course, but varies extremely in different cases both in course and duration, and in its progress from day to day. It is really a remittent fever, while the milder and subacute cases are intermittent in nature, and many of the so-called relapses are but the natural intermissions of the disease, which occurred even more frequently under the alkaline than under the salicyl treatment, as Dr. Owen's statistics have shown us. The knowledge that the symptoms are apt to recur is a reason for going on with the salicyl treatment till all danger of an intermission is over.

Hyperpyrexia is no essential part of the rheumatic process. It is one of its rarer complications, and is to be regarded as peculiar to the individual in whom it occurs, rather than as essentially pertaining to acute rheumatism. All evidence shows that it is not at all benefited by the salicyl compounds. They are quite inoperative in it. The external application of cold is the remedy for this condition.—*Lancet, Jan.* 14, 1882, *p.* 57.

14.—ON LEAD-IMPREGNATION IN RELATION TO GOUT.

By Dyce Duckworth, M.D., Assistant Physician to St. Bartholomew's Hospital.

In this contribution I shall embody my experience of the influence exercised by lead in cases of gout. The connection between lead-poisoning and gout is well established, although the nature of that connection is still but little understood. It is not too much to assert at once that the facts adduced on this subject by Dr. Garrod, first in 1854, and subsequently added to by him, have been fully confirmed by other observers, and the merit of fully setting forth the connection between the two disorders rests with him, although previous indications of it were made known more than a century ago. Dr. Garrod states that "at least one in four of the gouty patients who had come under his care in hospital had at some period of their lives been affected with lead, and for the most part followed the occupation of plumbers or painters." In 1870 he stated that 33 per cent. of people who suffered from gout had been poisoned with lead. These facts are very remarkable, and are probably insufficiently realised. Such an experience is not readily procurable, and London practice affords perhaps the only field in which such a study is possible on a similar scale.

The idea still prevails too largely that gout is a malady of the upper or well-to-do classes of society. More recent advances, so-called, in knowledge, have led to the belief that "poor man's gout" is a misnomer from the strict pathological stand-point, and should properly be termed rheumatoid or osteoid arthritis. I am very certain that such is not always the case, and I have no hesitation in affirming that true gout, in all its forms, is common enough in London amongst the lower and the working classes.

A distinction is to be made between "poor man's gout" and what may be termed "poor gout." Poor men may acquire gout by alcoholic intemperance, the latter, doubtless, often aggravating their poverty in several ways; but poor and rich men alike may become the victims of "poor gout," through no faults of intemperance of their own, but through the inheritance either of feeble constitutions or of true gouty tendency.

The connection between lead-influence and gout, whatever it may be, is naturally to be studied almost exclusively amongst the artisan classes, and therefore in hospital practice. Lead-impregnation is now, happily, very rare amongst the upper classes, owing to proper care in the storage and supply of potable water. Yet even here this influence should never be lost sight of in any case. Amongst the artisan population of London it is common to meet with cases of lead-poisoning, but the worst cases are seen in the workers in lead-mills. These persons are generally very poor, and only resort to this occupation when other means fail them. I find that they are often Irish, and that many women are amongst them, and it is not without importance to note these facts.

My own experience is taken from 136 cases of unequivocal gout in both sexes, which have been under my care during the last three years at the Hospital. Twenty-five of these patients, or 18 per cent., presented signs of lead-impregnation, and followed the occupation of painters, plumbers, compositors, or workers in lead-mills. They were all males. The age of the youngest was 25, of the oldest 62, the mean being about 43 years. In seventeen of the cases there were either present, or there were histories of, blue line, colic, and wrist-drop. In at least one-half of these patients there was history of intemperance, commonly in both malt liquors and spirits. In at least one-half the urine was slightly albuminous, of low specific gravity, and there were histories of cramps in the legs, and of nocturnal micturitions—all symptoms of chronic interstitial nephritis.

London experience certainly confirms Dr. Garrod's opinion that persons exposed to lead-influence are prone to develop gout, and that persons of gouty predisposition are specially liable to suffer quickly and severely from plumbism. It is remarkable that this experience should not be universal even in England. It is, however, very noteworthy that in many cases where true gout is not developed in connection with lead-impregnation, rheumatoid pains and arthritis are apt to supervene, and this fact appears to me to justify very fully the view held by Dr. Wynne Foot, Dr. Lancéreaux, and perhaps others, that the arthritis owns a neurotic origin. And this theory is specially acceptable to me, inasmuch as I am strongly convinced of the neurotic element in gouty disease in general, and believe it to be a neuro-humoral disorder.

A consideration of the physiological action of lead upon the body shows that both the nervous and circulatory systems are profoundly affected. Lead has been found in most of the tissues after death, especially in the brain and in the intestines. Gaffky believes that some change occurs in the vaso-motor nerves of the abdomen, especially in the sympathetic fibres of the splanchnic, by reason of which the renal mischief is induced. Kussmaul and Maier record a careful post-mortem examination of a case of chronic lead-poisoning, in which, amongst many other changes, they found proliferation and sclerosis of the connective tissue septa of the small ganglia of the sympathetic, especially the cœliac and cervical. These ganglia were hard, and the nerve-cells diminished. The smaller arteries were narrowed, and periarterial thickening was widely spread. It is not easy to follow the exact sequence or relation of these changes, but it is known that under the influence of lead the action of the heart becomes slow, and that the arterial tension is raised. It is also now well ascertained that a persistent condition of high arterial pressure is in itself a certain source of cardiac hypertrophy and arterial thickening, and it may well be that much of the mischief wrought by lead-impregnation is set up in this fashion, the particular form of kidney affection met with in this cachexia being associated with it. The presence of retained matters, such as uric acid, in the blood is certainly often associated with a condition of arterio-capillary fibrosis, and this impure blood has been supposed to meet with resistance in the smaller vessels, and to provoke higher arterial tension in consequence. It is, however, conceivable that this chain of events may result from injury primarily inflicted upon the sympathetic system of nerves by the contaminated blood.

Dr. Garrod has demonstrated that lead distinctly diminishes the secreting powers of the kidneys for uric acid, and Charcot likens this inhibitory action of the metal to paralysis of the kidney. The uric acid is consequently retained in the body. Due regard being had to these facts, it becomes easy to see a very close relation between lead-impregnation and the frequent occurrence of gouty manifestations. But it is not at once obvious why gout should not be more frequent in lead-cachexia than it really is. One or more factors in the causation are wanting. Dr. Pye-Smith has never met with gout from plumbism without hereditary predisposition or intemperance, and enough has been already shown to justify this assertion. We may explain the fact that gout is not found to be associated with lead-impregnation to any noteworthy extent in the North of England, in Ireland, and in Scotland, by a consideration of the conditions of heredity and of the drinking habits of the people in these various countries. There is, and there has been for centuries, more gout in the English metropolis, and amongst the beer-drinking inhabitants of the southern counties of England, than there is or has been amongst the populations in the North, in Ireland, and in Scotland, where spirits are consumed; and, therefore, it is only to be expected that the Southerners should yield the largest return of saturnine gout. On the other hand, beer-drinking is not alone in inducing gouty disease, for this will occur in persons who have been habitual spirit-drinkers only, and also in those who, as in the case of Parisian artisans, drink—immoderately, it is true—both brandy and inferior qualities of French wine. Hence we may believe that the two main exciting factors, inherited taint and intemperance, act often together, and sometimes singly, in producing saturnine gout.

The effect of lead in inducing gout must probably be attributed to the specific action of this poison upon the nerve-centres, this malign influence evoking such trophical changes in the entire vascular system, and in the kidneys, as are prone to be produced by the morbid condition we recognise as gout in its most comprehensive aspect. The lines of degeneration in the two affections, saturnism and gout, run, as it were, parallel, and seem only to be modified by individual habit and diathetic tendency.—*St. Bartholomew's Hospital Reports*, 1881, *p.* 249.

15.—ON RETINITIS HEMORRHAGICA, MORE ESPECIALLY IN ITS RELATIONS WITH GOUT.

By JONATHAN HUTCHINSON, F.R.C.S., Senior Surgeon to the London Hospital, &c.

The term Retinitis Hemorrhagica is not one which is beyond criticism as regards its appropriateness, because hemorrhages occur in various forms of inflammation of the retina and optic nerve. It has, however, come into employment as fairly descriptive of a group of cases in which very numerous hemorrhages, streaking the whole fundus of the eye, are the most prominent condition. In other cases the hemorrhages may be few and inconspicuous, or very large and almost solitary; here they are at once of considerable size and countless in number. They present usually another peculiarity, often absent in other cases, that they are all linear in shape, or, more correctly, flame-like, their points being directed away from the optic disc. Well-marked examples of the condition are not very common, and when once seen will be easily recognised. As a rule, there are none of the white streaks, dots, or patches which imply renal disease; but there is no reason why these should be absent in all cases, since albuminuria is not very infrequently present. The conditions are, however, very distinct both generally and locally, from those which we meet with in renal retinitis. In the best-marked cases there is always swelling of the optic disc and of the retina in its neighbourhood, but in others there may be comparatively little, and the multiplicity and form of the hemorrhages may be the chief feature of distinction.

You will find an excellent illustration of this disease in Jaeger's Atlas, a copy from which I now show you; and I produce also a good portrait from one of my own cases, which was done for me by Mr. Burgess. It is not my intention on the present occasion to trouble you with the details of single cases, but rather to invite your attention to some general facts suggested by a table which has recently been compiled for me, and which comprises all the cases that I can find in my note-books. To my own list I have added the case given by Jaeger, since an excellent narrative of the patient's history accompanies that portrait. The table which I now produce contains twenty-four cases. It reveals to us at a glance the fact that hemorrhagic retinitis is a disease of the middle or senile periods of life. Nine of the patients were above sixty, and not one was younger than forty-five. It shows a nearly equal proportion of the two sexes—thirteen men and eleven women. Most of them were at the time of the attack in apparently good health, but I know respecting several that death has since occurred from cerebral apoplexy or other causes. In not many had there been any previous evidence of tendency to rupture of blood-vessels (epistaxis, conjunctival hemorrhages, etc.), but it may be that I did not inquire in detail on this point in all cases. In connexion with this absence of general tendency to hemorrhage we may note that in only seven cases out of the twenty-four (less than one-third) were both eyes affected. In all the others the conditions were strictly limited to one eye. Respecting not a few of the cases, I can speak to the fact that the other eye remained free during the whole period that the patient was under my observation. This tendency to occur in but one eye suggests the probability that there must be some accidental locating or exciting causes in addition to those which predispose. We will return to that point presently.

I desire in the first place to say a few words as to the predisposing causes, and especially as to the association of the disease with the state of health which causes gout. It was indeed chiefly in reference to this latter point that I had the cases put together. My impression was that I had encountered a history of gout in nearly every instance of hemorrhagic retinitis which had come under my notice. I find, however, on counting the cases, that in seven out of twenty-four, or nearly one-third, such history was absent. Its absence, we must remember, by no means proves that the patients were not gouty, but merely that they had never experienced a gout paroxysm, nor known of such in any near relative. In counting seventeen as gouty, I do not exactly mean that all these had had acute attacks of gout, but rather that in all there was good reason to suspect a gouty constitution. In only twelve, or exactly one-half, had the patient actually experienced an attack of unmistakable gout. I count Jaeger's case as probably one of gout, though nothing definite is said on the point. The man was an innkeeper, and is said to have had a bloated face, and to have suffered from piles; and with three such facts we can scarcely doubt that gout was, at any rate potentially, present. Let me say that the recognition of gout as a cause of hemorrhagic retinitis, or at least as frequently met with in association with it, is not novel. Most writers on ophthalmic diseases mention it, though I am not aware that anyone has devoted much attention to the subject.

If we ask in what way gout acts in predisposing to this affection, several possibilities may be discussed. We know well that in gout there is a general tendency to disease of blood-vessels, thickening of arterial walls, and increase of pulse-tension. With this increase of tension there goes strain upon the capillaries and risk of rupture. This proneness to hemorrhage in the gouty has been noticed by several observers, and especially by Sir James Paget. Epistaxis and recurring blood-patches in the conjunctiva are both of them of frequent occurrence in the gouty. But it is not only the arterial system which suffers in gout: the veins also become dilated, and are liable to thrombosis; piles and other forms of varicose veins are well known to be common; and the patchy congestion of nose and cheeks not rarely seen is another piece of evidence in the same direction. Now, extreme turgescence of the veins is a conspicuous feature in most examples of this form of retinitis, and, although I do not know that I have ever been able to prove absolute arrest of circulation, yet in several I have believed that there was evidence of partial stoppage. An incomplete thrombosis of the vena centralis is, I think, no improbable hypothesis as to the immediate cause of the attack. It is especially probable in those cases in which the attack is sudden and only one eye affected. In many cases the attack is very sudden indeed, and, as I have already stated, in not a few the affection confines itself to one eye. Another suggestion to be made is that the heart itself is the cause of the attack, and that cardiac hypertrophy may be suspected whenever this form of retinal hemorrhage is observed. The suddenness of onset and the limitation to one eye are facts which strongly militate against this view.

I have said that hemorrhagic retinitis is never met with in the young; nor does it occur, so far as I know, in cases of hypertrophy of the heart in middle life. There is, however, in youth another form of intra-ocular hemorrhage, which, although rare, is well recognised. In it there is little or no inflammation of retina or optic nerve, but simply a liability to rupture of vessels. The bleedings are usually free and almost always escape from the retina into the vitreous humour. In this affection we never see the retina spotted over with flame-shaped streaks as in the cases we have been considering. A liability to sudden repetitions of hemorrhage is almost always a marked feature. My own experience had led me to believe that this affection, like the hemorrhagic retinitis of the aged, was usually seen in connexion with gout; but the facts collected by others, and especially by Mr. Eales, of Birmingham, have not borne out this supposition. It is found to be almost always attended by constipation and increased arterial tension. The fact that its subjects are always young lads, and never females, suggests the explanation that its attacks are in some way connected with that state of recurring vascular disturbance, which in the female sex finds its relief in menstruation.

In connexion both with these cases of intra-ocular bleeding in young lads, and with those of true hemorrhagic retinitis, we have the fact that the subjects of hæmophilia (the hemorrhagic diathesis in a definite sense) are usually liable to joint-pains, and often of gouty families, So strong are the facts on this subject that one writer has even contended that all hæmophilia is due to gout. It may be that the gouty state develops, as above suggested, a modification in the arterial coats, with increase of the risk of rupture, and that this structural peculiarity becomes specialised in hereditary transmission, and finally survives as a family peculiarity after all tendency to gout has, under altered circumstances of life, died out. A careful examination of the facts as to hæmophilia, with its most remarkable tendency to prevail in families, leads me to think that what I have suggested is by no means an improbable hypothesis. It begins in gout; it survives as a transmissible peculiarity of vascular structure. I fear you will think I am wandering from our subject. My object has been to impress upon your minds the fact that in gout there is a remarkable liability to hemorrhage from small vessels, and to disease not only of the central organ, but of both arteries and veins. The sum of our clinical knowledge respecting retinitis hemorrhagica appears to be this—that it never occurs till past middle life, that it is usually in association with gout, that it is often confined to one eye, and, I may add, that it is irremediable. For purposes of prognosis we must regard it much as we do attacks of epistaxis or of conjunctival hemorrhage under similar circumstances. It is a proof of degeneration of tissue, and may be the precursor of some form of apoplexy.—*Medical Times and Gazette, Dec.* 10, 1881, *p.* 675.

DISEASES OF THE NERVOUS SYSTEM.

16.—ON THE "PATELLAR-REFLEX" PHENOMENON, WITH ILLUSTRATIVE CASES.

By JOHN W. BYERS, M.A., M.D., Physician to the Hospital for Sick Children, Belfast.

In the year 1875 two celebrated German observers, Westphal and Erb, directed the attention of the profession to the great value, from a medical point of view, of a phenomenon long known as a schoolboy's trick. I refer to the peculiar jerk of the leg which takes place when the patellar tendon is struck. Charcot may have used the patellar-reflex in diagnosis before this period, but undoubtedly the profession at large was first made aware of its importance by the appearance of the papers of Westphal and Erb.

Since then the phenomenon has been studied with extraordinary care in its physiological, pathological, and clinical bearings by Westphal, Erb, Tschirjew, Burckhardt, Fürbinger, Schultze, Berger, Eulenburg, Charcot, Brissaud, and Franck on the Continent, and in this country by Grainger Stewart, Hughlings Jackson, Waller, Buzzard, and Gowers; so that, I think, no one will accuse me of exaggeration when I say that there are few symptoms of greater importance in the diagnosis of diseases of the nervous system than this patellar-reflex phenomenon, especially when it exhibits deviations from its normal condition.

The phenomenon has received many different names; it has been called "the patellar-tendon reflex phenomenon," "the knee-jerk," "the knee-reflex," the "knee phenomenon."

In this paper I shall—First, describe the phenomenon as it exists in a healthy individual. and mention several methods that may be employed to elicit its presence. Secondly, I shall state and examine the different views that have been propounded as to its physiological causation. Thirdly, I shall read the notes of a series of cases in which this phenomenon exhibited deviations from its normal condition, and I shall attempt in each case to account for such alteration.

First. You see in my friend (one of the hospital pupils) who is sitting with his right leg crossed over the left, and bent at an angle slightly greater than a right angle, that when, with this stethoscope, which has an india-rubber ring round the pectoral piece, I strike the right leg just below the patella, immediately the foot is jerked forwards. Such is the phenomenon, and such is the ordinary method of testing for its presence.

The following plan is suggested by Buzzard if it is desired to ascertain the mere presence of the "patellar-reflex:"—"The patient, who is seated, plants his foot firmly down at such a distance that the leg forms a little more than a right angle with the thigh. Whilst the observer rests the palm of his left hand upon the patient's thigh, he strikes with some implement held in the right hand several blows upon the ligamentum patellæ at about an eighth of an inch or so below the knee-cap. The quadriceps muscle can be felt, and (if the patient is in masculine garments) can be seen to contract more or less vigorously in response."

The side of the hand can be employed as a percussor. If no response follows the blow the leg should be bared, and the observer should test most carefully by striking in the usual place, or on the patella, or the tendon above the patella, or the muscle itself, before assuming that the reflex is absent.

In children, in whom there is often considerable difficulty in examining for the sign, I usually place them on a table with their legs hanging over the side, and while their attention is occupied in looking at a toy, a light tap on the patellar tendon is usually sufficient to demonstrate the presence of the phenomenon. The patellar-reflex is almost always present in health, although the amount of the contraction of the quadriceps muscle may vary at different times even in the same individual. Berger has examined 1,409 soldiers for this phenomenon, and has found it absent in 22, or 1·56 per cent. of these cases.

Although I have seen the patellar-reflex slight in several individuals, and absent in one—while in none of these when examined was there any evidence of disease—still I think that such cases are exceptional; and I am inclined to believe that the greater the care that is taken in testing for the sign the fewer will be the number of those in whom it is found to be naturally absent. With reference to those cases in which it is stated that the phenomenon is normally absent, it is of little importance, as Westphal thinks, to note in the case of any one that at a given time the patellar-reflex is absent, more especially if we know nothing of his history. The observation would be of much greater value if we could show that the phenomenon *had* disappeared. In some of these cases the absence of the patellar-reflex may be a premonitory sign of a spinal affection of which other symptoms will appear in the future. Again, in certain individuals the absence of the phenomenon may be owing to mechanical causes, Westphal having shown that it is difficult to develop the patellar-reflex in those in whom the tendon is very short, in those in whom there is a thick pad of fat under the tendon, and in those who are very stout.

Secondly. We shall now consider the physiological causation of the phenomenon. Two views have been suggested to account for it. According to one it is purely reflex—that is to say, that at the point where the observer strikes the leg, impulses are generated which travel up the sensory nerves to the cord, at about the level of the second and third lumbar nerves, and there undergo certain changes, of whose nature we are ignorant—except that as a result efferent impulses descend the anterior crural nerve to the quadriceps muscle, causing it to contract and to jerk forward the foot. According to the other theory the phenomenon is a local one, due to a direct irritation of the quadriceps muscle.

In support of the first view, advanced originally by Erb, and according to which the reaction is of the nature of a spinal reflex, evidence is forthcoming from two sources—(*a*) the experimental, and (*b*) the pathological.

(*a.*) As a result of experiments on animals (Fürbinger, Schultze, Tschirjew, Senator), in many of whom the patellar-reflex exists, it has been found that destruction of the portion of the cord in the lumbar region, with which the anterior crural is connected, or of the anterior or posterior roots of the spinal nerves in the same situation, or of the nerves to the muscle, is followed by a disappearance of the phenomenon.

(*b.*) In any disease in which the reflex nervous arc, at the situation mentioned, is injured, in either its sensory or motor portion the patellar-reflex is lost. This is exemplified in tabes dorsalis (Case I.), and in poliomyelitis anterior acuta (infantile paralysis) (Case III.)

In favour also of the reflex theory is the fact that drugs which increase the irritability of the cord — *i.e.*, strychnia, cause an exaggeration of the patellar-reflex; thus Eulenburg, in some cases in which the phenomenon seemed entirely abolished on one side, succeeded in making it reappear for some time by the hypodermic injection of strychnia, made in the neighbourhood of the nervus cruralis, or into the substance of the extensor muscles. On the other hand, medicines, such as bromide of potassium and opium, which diminish the irritability of the cord, lessen the activity of the phenomenon. Admitting, then, that the contraction of the quadriceps is of reflex origin, the question naturally arises—From what point do the sensory impulses arise when the leg is tapped; is it from the skin, the patellar tendon, or the muscle? That they do not begin in the nerves of the skin is clear from the following facts:—1. A light tap or push over the patellar tendon will not give rise to the knee-jerk. 2. If the skin is lifted up in a fold, so as to be quite separate from the tendon, and is then tapped, no contraction of the muscle follows. But, on the other hand, if the skin is divided and the blow struck on the exposed tendon the leg is immediately jerked forward; this experiment seeming to show that the irritation starts from the tendon—a view that gets additional weight from the circumstance that Sachs has demonstrated the existence of nerves to the patellar tendon. But this view—viz., that the impulses arise in the nerves of the patellar tendon, is disproved by an experiment of Tschirjew. This observer divided all the nerves to the tendon patellæ in an animal, and, notwithstanding, a blow on it caused the muscle to contract. Further, in all cases in which a blow on the patellar tendon causes the muscle to contract, it must be in a condition of passive tension, and in some cases, as Erb has shown, *sudden* tension alone, as by quickly pushing down the patella, so as to make the quadriceps tense, will cause the appearance of the knee-jerk. Dr. Gowers has also shown that in certain cases in which the "tendon reflexes" are in excess a tap on the front of the leg will cause a movement of the foot. Here the stimuli do not act on the tendon but on the muscle.

So then we are compelled to believe that the stimuli arise in the muscle. But if this be granted Westphal's view that the contraction of the muscle is a local one may, after all, be the correct explanation of the phenomenon; and this theory is supported by the fact that Burckhardt, Brissaud, Tschirjew, Gowers, and Waller, who have examined the matter with great care, believe, as the result of their investigations, that the time which elapses between the blow on the tendon and the contraction of the muscle is shorter than it should be if the phenomenon was a purely reflex one. At the recent meeting of the International Medical Congress, in a discussion on his paper, Eulenburg gave his adhesion to the view that it might be a peripheral phenomenon. But if we adopt this explanation, how can we account for the absence of the phenomenon in those diseases which cause destruction of the reflex nervous arc?

Is there, then, any hypothesis which will reconcile these two opposing views, which will account equally well for the contraction of the muscle being a local one, and will yet explain the circumstance that any lesion which destroys reflex action causes a disappearance of the knee-jerk? The following explanation is given by Dr. Gowers:—"If," he says, "we regard the contractions as local we have still to account for the irritability which permits the local stimulus to cause a contraction. This irritability is developed by passive tension. Hence I have suggested that the tension excites, by a reflex influence, a state of extreme irritability to local stimulation—such as that of a tap on the tendon, or such as the vibration from a tap near the muscle, or from a tap on the bone to which the tendon is attached—which thus excites a visible contraction."

I have endeavoured to give a full and fair account of the various views that are held as to the cause of this very remarkable phenomenon. Whatever view will eventually be found to be correct, the fact remains that this patellar-reflex phenomenon is of great value in disease, examples of which I shall now give in this the third part of my paper.

Case 1.—*Tabes Dorsalis; characteristic Ataxic Gait; Argyll-Robertson Pupil; absence of Patellar-reflex in both Legs; previous history of Pains in the Legs, attacks of Indigestion, and Diplopia.*—In Sept. 1881, I was consulted by a gentleman, aged forty-two, who gave the following account of his illness:—He was in perfectly good health until the year 1878, when he suffered from attacks of indigestion, accompanied with flatulence, especially after food. These attacks were not attended with vomiting, but during one of them he was much surprised one day, in looking out of the window, to find that each person passing along the street seemed to him to be double. This double vision was merely a temporary annoyance, but the attacks of indigestion have recurred at intervals up to the present time. In 1879 he had pains in "the lower part of his back," of a dull, heavy character, and in the spring of the following year he had also pains in his legs, specially in the back of the right leg, but also, though to a less degree, in the left. These pains have not been of a shooting character, but more like "rheumatic" pains. In the beginning of the year 1881 he first became unsteady in walking, and this unsteadiness was always worse at night. This was noticeable even to his friends at this time, for he states that one evening in coming out of the Turkish baths into the street his walk was so peculiar that a gentleman who was with him said, "You are like a drunken man, or are falling into paralysis." From this time his heels in walking always touched the ground first. In the mornings, while washing his face (his eyes being then closed), he has a tendency to fall forward. Even before his movements became unsteady he had difficulty in going up and down stairs, and was obliged to keep a firm hold of the baluster. For some time he has had a curious feeling in his feet, "as if there were two or three folds of the sock under the sole of each foot." He never noticed anything peculiar about his eyelids or eyes, with the exception of the double vision which I have mentioned.

Present Condition.—He is a very intelligent, large, well-developed man. He walks in the most characteristically ataxic manner, lifting his feet high, and bringing his heels down on the ground with a thump. He has great difficulty in starting to walk and in turning, his movements at these times being very unsteady. When he brings his feet together, and closes his eyes, he begins to sway from side to side, and would fall if not supported. In both legs, from the knee downwards, he feels the prick of a pin, and a heated knife when in contact with the skin, merely as tactile sensations. There is no wasting of the muscles of the legs, and when lying on a sofa he moves the legs freely in any direction. That the tabetic incoördination has extended to his upper extremities is shown by the apparent difficulty he has in buttoning his waistcoat. His pupils are very small; they do not respond to light, but act during accommodation (Argyll-Robertson symptom). There is no ptosis or strabismus. He has great difficulty in distinguishing blue from green wool. There is no visceral mischief of any kind to be detected in the chest or abdomen. Patellar-reflex absent in both legs.

In the above case, when seen by me, the diagnosis was so clear that the absence of the patellar-reflex was of assistance principally in corroborating the other symptoms; but the great value of the symptom is in the early stages of tabes dorsalis, before the ataxic gait has become developed, and then it is frequently associated with the lightning pains. Of the very great importance of the absence of the patellar-reflex in tabes dorsalis, Dr. Buzzard, one of the most recent writers on that disease, thus speaks:—"Let me say then at once that, in my judgment, we may take it that Westphal's test, the absence of the knee-phenomenon, provided that this is associated with good voluntary power and ideo-muscular contraction of the vastus internus—a provision which Dr. Erb has most strongly and justly insisted upon—is a nearly certain sign of tabes dorsalis. I am prepared indeed, from something which I lately saw, to meet with the absence of the knee-phenomenon as possibly representing the only symptom of tabes in a certain case."

In fifty cases examined by Erb the patellar-reflex was absent in all but one, and he thinks that it is probably always an early symptom. In his experience it is never absent in young or middle-aged people in health, and hence he thinks that its absence may be regarded as almost pathognomonic of tabes in its earlier diagnosis.

The lesion in tabes dorsalis is sclerosis of the posterior columns, or, to state the matter more correctly, of that portion of these columns known as the "posterior root-zones" (Charcot), involving the posterior roots on their way to the posterior cornua. This lesion causes an interruption of the sensory impulses in their passage to the spinal centre, and so the patellar-reflex is lost. Even without our knowledge of the morbid anatomy of tabes, we would almost be warranted in concluding that the motor portion of the reflex arc was intact, from the fact that good voluntary movement of the lower limbs is preserved, and absence of any wasting of the muscles; while, on the other hand, that the sensory portion of the loop is affected, is apparent from the lightning pains and the anæsthesia.

Westphal thinks that there is a particular part of the posterior columns which, when diseased, causes the disappearance of the patellar-reflex; and, as bearing directly on this question, I will give a short *resumé* of a case, an account of which he brought before the Medical Society of Berlin.

A man, thirty-two years of age, in whose family there were numerous examples of different kinds of nervous affections, was brought in 1877 to the Charité Hospital, Berlin. He had been blind for six months, being merely able to tell, and that only with his left eye, day from night. He was a pronounced hypochondriac, with all sorts of hallucinations. He had no symptoms of tabes, neither irregular movements, nor anæsthesia, nor lightning pains. In October, 1879, the patellar-reflex disappeared on the right side. He gradually got worse, and died in January, 1880. A few days before his death the "patellar-reflex" was gone on the left side—it continued absent on the right.

At the autopsy, in addition to very advanced degeneration of the optic nerves, optic tracts, and corpora quadrigemina, a yellow zone or strip of degeneration was seen in the centre of the posterior columns of the cord distinctly marked off from the neighbouring parts. The situation of this region of degeneration in the posterior columns was altogether peculiar. It attained its maximum on a level with the lumbar part of the cord. The cornua, columns of Goll, and posterior roots were intact. Westphal infers from this very unique case that the loss of the patellar-reflex in tabes dorsalis is due to a lesion involving a definite zone in the centre of the posterior columns of the lumbar part of the cord. This observation of Westphal is particularly interesting as probably representing in some cases of tabes the first pathological change in the spinal cord, and as explaining the early absence of the patellar-reflex. Charcot had previously been led to assume that the sclerosed bands in tabes are slender and narrow so long as the lightning pains are the only symptoms present; as these areas of degeneration enlarge both outwardly and inwardly, the tabetic incoördination is observed. If there is marked anæsthesia, the posterior roots and posterior gray cornua are involved; while, finally, if the sclerosis extends to the posterior part of the lateral columns, the clinical picture is represented by paralysis (with or without contracture), the advanced stage of the malady. As to those cases of tabes in which the patellar-reflex is said to be present, Westphal thinks that some of them are not genuine cases of tabes, while in the case of others it is possible that the patellar-reflex may disappear late in the course of the disease, indicating that the central portion of the posterior columns has also been involved at a late period.—*Dublin Journal of Medical Science, March*, 1882, *p.* 186.

[Dr. Byers gives three other cases to illustrate this interesting paper.]

17.—ON SOME CURABLE AND RELIEVABLE FORMS OF ADULT SPINAL PALSY.

By W. Carter, M.D., LL.B. Lond., Physician to the Royal Southern Hospital, Liverpool.

In his work on Localised Electrisation, Dr. Duchenne mentions cases of what he terms acute and sub-acute anterior spinal paralysis. These cases are characterised by loss of power, with retention of sensation in the limbs, and, unless recognised early and treated properly, by rapid wasting of the muscles. They differ from the paralyses which follow fracture or dislocation of the vertebræ, from those that result from myelitis following strain, tumour, &c.; and, in brief, from the paralytic results of "irritative lesions occupying either the central and posterior portions of the grey matter, or the posterior white fasciculi" of the cord (Charcot) in not being attended by bed-sores or loss of control over the bladder or rectum; and, in general, from all forms of acute, general, or diffuse spinal paralysis in not being of a directly fatal tendency. The ultimate anatomical lesion on which the paralytic symptoms and the wasting depend is almost certainly, as in infantile paralysis, atrophy of the cells of the anterior cornua. Probably, however, there is a stage anterior to that in which wasting of cells takes place, a stage easily recognisable by symptoms, and more immediately controllable by treatment. Speaking broadly, the earlier symptoms very much resemble those following an artificially induced hyperæmia of the spine and cord, and consist of increased sensibility, a feeling of general soreness, local or general pains, muscular twitchings and tremors, diminished bodily temperature, &c. Accompanying or quickly succeeding such symptoms come those of extreme fatigue on undertaking the slightest exertion, even that of standing; then more or less paralysis, and lastly wasting, often very rapidly developed, of the muscular system.

I have records of some few of these cases, four of which, three in hospital and one in private practice, came under my notice in the acute stage, and were permanently cured; the remainder being chronic and the cure being partial. I do not include in this list cases where groups of muscles only have become wasted owing to some undue strain put upon them, or other similar cause. The following are examples:—

Elias Iverson, a sailor, aged 41, was admitted into the Royal Southern Hospital under my care, suffering from dysentery, on 9th February, 1879. His ship having sprung a leak some few weeks previously, he was compelled to be at the pumps night and day for four hours at a time, so long as he was able to stand. For a fortnight before his admission he was confined to his berth from dysentery, and on the ship reaching Liverpool was sent at once to hospital. This was his fourth attack of dysentery. On admission he was suffering from great straining and pain, and was passing from twelve to fourteen stools daily, each stool containing blood.

The urgent symptoms were soon subdued, and on the 14th of March he got up for the first time. When up he complained of a feeling of soreness all over the body and extremities, which he had not experienced when he was lying in bed. He was ordered half-drachm doses of liquid extract of ergot with the compound hæmatoxylum mixture of the Guy's Pharmacopœia four times daily. One stool daily was passed without blood for several days, but in addition to the feeling of soreness he complained of great weakness, and was affected by tremors on endeavouring to make any exertion. On 24th March he had rigors, followed by sweating. The muscular weakness so increased that on the 29th he could not sit up in the bed. His pulse was 116, weak. Temperature normal, or somewhat subnormal, ranging from 97·2 to 98·8. On the 31st he tried to sit up, but his head fell forward on his chest. By the dynamometer the right hand registered 34, the left 32.

As he lay in bed the pulse was 120, but sitting up for a few moments was attended by the following symptoms:—The pulse became more rapid, smaller, scarcely detectable at the wrist; the face became extremely pale; dizziness was complained of, and the head fell forward with the chin upon the chest, from want of power in the splenii, complexi, and other extensor muscles. He could not extend the arms horizontally forwards, nor cross either leg over the other. There were constant minute muscular tremors in the arms and legs. Strychnia, in $\frac{1}{16}$ gr. doses three times daily, was prescribed. Directions were given to shampoo the limbs without their being exposed to cold. The continuous current from twenty-five cells was passed for five minutes twice daily along the spine, whilst the limbs were Faradised for the same length of time daily.

On the 18th he could cross either leg over the other, could raise his right arm vertically and bring it down to his head, though the effort caused much tremor, and could extend it horizontally forward for two or three seconds. At the end of that time it would fall, and, while it was extended, the scapula was drawn away from the chest wall, and its lower angle tilted so far backwards that a deep hollow was left into which the hand could be passed. The dynamometer registered 45 and 46 with the right and left hand respectively.

On the 8th April the strychnia was increased to $\frac{1}{10}$ gr., and in addition 5 m. of the officinal solution ($\frac{1}{24}$ gr.) were injected in each shoulder alternately daily, and $\frac{1}{32}$ gr. of phosphorus was given in a pill three times daily. By the 12th he could manage, though with great difficulty, to put his shirt on without assistance. On the 26th he walked about half a mile, though the exertion caused him very much fatigue.

On the 28th the amount of strychnia hypodermically injected was increased to $\frac{1}{12}$ gr. The improvement was progressive. On 2nd May a note was made that there was much less pain in the legs, though walking was still followed by a great sense of fatigue. By the 18th the dynamometer registered 62 and 60 for the right and left hand respectively, and as he was in every respect greatly improved, and able to walk about without much fatigue, he was sent on the following day to the Woolton Convalescent Institution, whence in a few weeks he returned well.

The next case also came under observation soon after the symptoms made their first appearance. C. B., a very intelligent young man, who just before his illness had been employed as a clerk to a shipping firm, first complained of slight stiffness of the legs on 25th February, 1880. Thinking it was the result of a cold, and would pass off, he did not consult any medical man. The stiffness gradually increased during the next few days, and on 2nd March, being alarmed by this, and by his inability to whistle, when trying in that way to hail an omnibus, he sent for Dr. Preston of this city, who, by the consent of his late employers, requested me to see him in consultation on the 6th. He was admitted to hospital on the 15th of the same month, in nearly the same condition as when I first saw him. His speech had rapidly become defective and difficult owing to double facial paralysis. Any attempt to utter words containing labials or *f*, *ph*, *v*, or *g*, puzzled him exceedingly. The ordinary lines of expression had disappeared from each cheek. He could not close either eye (though the lids of the left one were more nearly approximated than those of the right), nor could he blow out the cheeks. There was much diminution in the contractility of the uvula and soft palate, but fluids did not return.

through the nostrils as they were reported to have done occasionally before admission. The special senses were unimpaired, and the optic discs, when ophthalmoscopically examined, presented a normal appearance. Muscular power in the arms, hands, and legs was very much diminished. The dynamometer registered 20 for the right and 25 for the left hand. He could perform all ordinary movements with his hands and arms, though all the muscles presented on a close inspection minute fibrillary tremors; and when the fingers were extended and separated they were seen to be in constant lateral motion, owing to the irregular action of the interossei and lumbricales. He was quite unable to stand, or, when supported in a sitting posture, to cross either leg over the other. Patellar tendon reflexes were absent. As he lay in bed on his back, with the legs flexed, a very small counter pressure on the soles of the feet was sufficient to prevent his extending them. He complained of "pins and needles," and other abnormal sensations in his hands, and of numbness in the toes, but generally common sensation was not markedly impaired. On examination a good deal of tenderness was complained of when pressure was made over the temporal muscles, and at a spot on either side one inch above and behind the ear. The bowels were constipated, and the tongue unequally covered with patches of thin white fur. Action of the bladder was under control, but the urine was not passed in a vigorous stream. Respiration, pulse, and axillary temperature was respectively 20, 94, and 98° Fahr. soon after admission. Heart and lungs normal.

The bowels were opened by compound colocynth pill and podophyllin, and the following special treatment ordered:—A weak continuous current, six cells, across the base of the head, and along the spine for five minutes daily; and a half-drachm of the liquid extract of ergot in water to be taken three times a-day. Milk diet.

This treatment was continued for some time, and he soon began to show signs of improvement. First, the grasp became stronger, then the numbness of the toes and the abnormal sensations in the arms became much diminished, and by the 13th he could rise from the couch without assistance and walk two steps. On the 19th he walked several paces from his bed and back again without assistance, and one week later walked across the ward and back, though this was followed by a feeling of intense fatigue. On the 28th a number of points with the white-hot cautery were applied along each side of the spine, the cautery being immediately followed by the application of a large damped sponge, after which a towel was stretched along the back and friction applied for some minutes until two tracks of vivid redness were produced. On the 18th he could draw up the upper lip on the left side so as to expose the teeth, though not on the right. The dose of ergot was increased on this day to one drachm and a half, and 15 grains of iodide of potassium added. Improvement continued, and on 3rd June he left for his home in Suffolk. An examination made on the 31st of May gave the following results:—He could walk fairly well without any dragging of the limbs, the grasping power was good, he could write very distinctly and with facility, and could easily throw either leg over the other when sitting; the patellar reflexes were present, the left more pronounced than the right. He could quite close the left lids, but not quite the right. He could blow out the cheeks without air escaping. The right side of the upper lip could not be raised so well as the left, and there was still some difficulty with such words as "facetious."

I had several letters from the patient since his discharge, in the last of which, written on 27th July, he said that he could walk five or six miles without fatigue; while on the 6th of the same month he reported that he could quite close the right eye, that speech was but "very slightly affected," and that no difference between the right and left cheek was perceptible to a casual observer.

As to the treatment of these cases, which after all is the most important matter for the patient, the following seem to me to be the most rational principles to act upon, and were kept in view in those that have come under my care:—In the acute stage, a stage having probably as its anatomical character a condition of hyperæmia of the chord, and marked by exalted sensibility; fibrillary tremors; and sometimes twitchings of muscles; disturbed areas of sensation, as if, for example, a hot metal plate were held close to some portion of the skin; a feeling of the most intense fatigue of limb and back on undertaking even a slight exertion (the natural healthy colour of the face being all the time but little, if at all changed); means calculated to diminish hyperæmia are most useful, and of these means the internal administration of ergot and the employment of counter-irritation, such as that by blisters or Charcot's white hot cautery along the spine are perhaps the best. But so soon as this stage has passed, and paralysis and wasting have set in, means having for their object to nutrify the nervous centres and the muscles should be adopted. Of these means, strychnia by mouth and hypodermially injected, phosphorus, Faradism and shampooing of the muscles, and the constant current to the cord, are perhaps the best yet known, and the persevering employment of them will be often followed by immense benefit, even in cases of long standing and great severity.—*Liverpool Medico-Chirurgical Journal, Jan.* 1882, *p.* 27.

18.—ON PERCUSSION AS A THERAPEUTIC AGENT IN NERVOUS DISEASES.

By J. MORTIMER GRANVILLE, M.D.

With the cognisance of the leading physiologists and neurologists in England and on the Continent, I have for some years past been employing carefully graduated and precisely applied percussion as a therapeutic agent in the treatment of nervous diseases and disturbances, on a principle of which the following statement, published by me in February, 1881, may be taken as a brief exposition.

"As far back as 1862-3, I was, in the course of certain clinical studies of mental and sensory phenomena, induced to believe that many forms of the sensation we call "pain" were, in fact, unnecessary, and might be interrupted by appropriate mental and physical methods and appliances. My first observations were made in connection with the paroxysmal or recurrent pains accompanying the uterine contractions in the natural process of parturition. On May 4th, 1864, Dr. Graily Hewitt was good enough to communicate the results of my experiments and to show certain apparatus, to the Obstetrical Society of London. In a paper On the Application of Extreme Cold as an Anodyne in the Pain attendant on Parturition, a short abstract of which will be found in the Lancet of July 9th, 1864, I contended that the sensations of pain experienced by the parturient woman were not invariably synchronous with what, for want of a better name, we term the "pains" of her labour; and from this and other premises—for example, the circumstance that the sensation is commonly "referred" to some region more or less remote from the contracting uterus, or the dilating external passages, in which the real seat of the pain might have been supposed to be located—I deduced that the pain attendant on labour is neuralgic in its character. I had constructed small boxes or chambers of such sizes and shapes as to admit of their being conveniently applied to the supposed seats of the pain. These were filled with freezing mixture, and the effect of sudden contact in some thirty cases was to arrest the sensation of pain without in the least degree lessening the force of the uterine contractions. The experiment was, of course, simply interesting as bearing on the nature of the pain, as this process was too troublesome to admit of its adoption in practice; albeit some of the persons on whom I had the opportunity of trying my method experienced such striking relief that, on subsequent occasions, I believe they asked that the measure might be repeated. Having thus far persuaded myself that this form of pain was neuralgic, and that if the nerve affected could be strongly impressed, so as to change its *state of irritation*, the pain would cease, I proceeded to try the effect of rapidly tapping the skin over the fifth nerve in ordinary facial neuralgia with a Bennett's percussion hammer, using the ivory pleximeter as a shield. The results obtained by this method were very remarkable. Still, I simply thought of arresting the morbid action by shock. Later on—it is only possible to sketch the outline of the inquiry—I was led, by the light thrown on Newton's doctrine of concords and discords by Grove's generalisation as to the correlation of forces, and, more recently, by Professor Tyndall's beautiful series of experiments with sensitive flames and musical burners, to believe that the results of the tapping were not, like the interruption with shock produced by the sudden application of cold, due to a mere arrest of the painful state of irritation into which the nerve had been thrown, but were, in fact, brought about by the extinguishment of some morbid—that is, either inordinate or disorderly—set of vibrations by the superimposition of another, incompatible or discordant, set of vibrations mechanically produced. With this notion, I set to work to devise an instrument which should give a known number of blows per second, and thus admit of this new phase of the inquiry being pushed further . . . .

"The sensation produced by the application of the instrument over a healthy nerve, so situated as to be readily thrown into mechanical vibration, closely resembles the effect of a weak dose of the interrupted current of electricity, and if it be prolonged the vibration will extend its area, exciting first formication or tingling, then a sensation of numbness, and finally some twitching of the superficial muscles. A nervous headache, and even migraine, may be induced by the application of the percuteur to the frontal ridges or the margins of the orbit. By the interposition of a thin plate of metal, or even stiff paper, the vibration may be readily propagated through a considerable region of the surface of the body, and in time the deeper muscles will frequently begin to act. I have even produced an involuntary movement, not unlike tendon-reflex, by applying the percuteur for some time over the ligamentum patellæ or the margin of the patella. Still more notable has been the fact that, by laying a sheet of paper over the abdomen, and moving the percuteur slowly in large circles round the umbilicus, the intestines have seemed to be excited to vermicular movement, and the bowels commenced to act. These results have not been constant, but have occurred with sufficient frequency to indicate that the experiments already made are worthy to be repeated.

"I will take leave to say that I think these results go to support my theory that it is by the introduction of discord into the rhythm of the morbid vibrations of the painful "state" the change which brings relief in neuralgia is effected. To apply the percuteur with a high rate of blows per second will aggravate the morbid state when that is itself a series of rapid vibrations; and in the same way a low speed of percussion increases, instead of relieving, the pains of a low-pitched and slow "boring" or "grinding" sensation. Acute or sharp pain is, I believe, like a high note in music, produced by rapid vibrations, while a dull, heavy, or aching pain resembles a low note or tone, and is caused by comparatively slow vibrations. A slow rate of mechanical vibration will, therefore, interrupt the rapid nerve-vibration of acute pain, while quick mechanical vibration more readily arrests the slower. The aim—if I am right in my conjecture—should be *to set up a new set of vibrations which shall interrupt or change the morbid set by introducing discord.* This is the principle. Failure in the application of this principle will, I believe, be found to explain the failure to put an end to the pain; and I have, accordingly, set as much scientific value on my failures as on my successes."

My method is, it will be seen from these extracts, based upon the hypothesis (1) that all nerve action, whether normal or morbid, is vibratile; and (2) that it is possible to influence and control abnormal vibrations—in the manner above described—by mechanical vibrations propagated to the nervous structures, in *particular* directions and at *known* rates of speed. It is not my present purpose to discuss these hypotheses, or the method in detail; but I am anxious to re-state, and now affirm, certain propositions, founded on experience, which, in previous intimations of the progress and success of my experiments I submitted tentatively. They are these.

1. I have rarely failed, in a fairly large number of cases—many of them of several years' standing—to bring the cerebro-spinal and, sometimes directly, at others in secondary circuits, the sympathetic ganglia under control, by the application of my percuteur over, or in mechanical relation through the adjacent tissues with, those ganglia.

2. I have in no instance failed to produce activity of the bowels, even in cases of previously obstinate constipation; and in many instances I have succeeded, within a short period, in restoring the periodic evacuation of their contents without recourse to drugs. This success alone places the method on a footing of value in daily practice.

3. I can now, in result of my more recent experiments, propagate the vibrations I produce along the trunks and into the branches of most of the principal nerves, from their centres of origin, or call them into action, reflexly, through the afferent nerves connected with those centres. In limited paralyses, and even in circumscribed scleroses, this power is of the highest therapeutic importance.

4. I can nearly always arouse torpid centres to action, and

thus pave the way for their restoration to states of normal activity. Since it is physiologically certain that nutrition depends on exercise, and every part of the organism feeds in proportion as it works *healthily*, it is a great thing to be able to act thus directly on the nerve-centres which are the seats of energy.

5. I can subdue the exaggerated reflex irritability of revolting subordinate centres, and replace them under the control of the higher centres, even in cases of lateral sclerosis.

Applying these facts—for such they undoubtedly are—to the needs of special nervous states, the practioner will have no difficulty in perceiving that my method has great and obvious uses. I am anxious not to overstate the results I am obtaining, but they are such as to show that the physiological process of mechanical vibration is likely to prove a potent agent in the treatment of a wide range of maladies now the most intractable. It will afford me much pleasure to show the process to any medical man who will call on me. It is impossible at present to describe its details in writing; but I will gladly aid anyone in its application. My method has nothing in common with the "muscle-beating" and shaking to which you directed attention in your last issue. It is a system which must be approved and practised by the profession exclusively. Nothing do I so much dread as its falling into unprofessional hands. I have been engaged upon it since 1862, shortly after which date some of the first results were communicated to the Obstetrical Society.—*British Medical Journal, March* 11, 1882, *p.* 339.

19.—A CASE OF ACUTE TRAUMATIC TETANUS, TREATED SUCCESSFULLY BY CHLORAL AND BROMIDE OF POTASSIUM.

By J. H. Salter, Esq., Tolleshunt D'Arcy, Essex.

Isaac Hume, aged fifty-one, labourer, married, living at Goldhanger, Essex, smashed the little finger of his right hand. It was first poulticed, then dressed with water-dressing, and seemed disposed to heal. Shortly afterwards an abscess appeared on his foot, which was likewise poulticed and healed.

About seven weeks after the accident to his finger he was attacked with pains in his neck, back, and jaw, which came on in paroxysms, not very severe, and were attributed to rheumatism. By and by the flexors of his trunk and extremities were involved and his jaw became rigid and fixed. Symptoms continued to get worse, the intervals between the pains shorter, and the jaw more closely locked.

I then saw him (the date being 22nd April, 1881) and recognising the case to be one of tetanus, with distinct "risus" and increasing opisthotonos, I determined at once to treat him with chloral, as I did a similar case in 1875, the particulars of which appeared in the Practitioner for Dec. 1876.

The pains steadily increased in severity and frequency—the average interval between them being about twenty minutes on the first day I saw him. Lying down made them worse: the easiest posture was the erect. He always begged to be placed on his feet when he felt a spasm coming on, and to be held there during its visit. In this position, therefore, he was put on the approach of pain, whether night or day, throughout his illness. He could swallow fairly; but it more often than not induced a spasm of variable intensity. His temperature was normal throughout. His pulse, except as it responded to his gradually weakened condition, exhibited nothing worth noting.

I began by giving him steadily, chloral in ten-grain and bromide of potassium in fifteen-grain doses, every two hours, increasing the frequency of the dose as the spasms were more frequent or severe. In addition to this I usually gave him from five to ten grains of chloral hypodermically whenever I called to see him, which, during the early part of the time, was three or four times in the twenty-four hours. These hypodermic does quieted him much more markedly than those taken by the mouth, and the influence of the drug remained for a longer period. The bowels acted without much trouble, now and then requiring assistance by an ordinary aperient or by injection. The urine was always loaded with lithates, and scanty; perspiration being remarkably free, especially during the painful paroxysms.

He was fortunately able to swallow, in small quantities, a great deal of nourishment, consisting of strong meat broth, raw eggs, and new milk; but I withheld stimulants in the early days, reserving them until I found, by pulse indications, that they were required.

On examining his injured finger at my second visit (on 23rd May), I found the terminal phalanx loose and projecting from the wound; I removed it immediately and applied a poultice; but there was never any sign of pus, and the wound from the beginning to the end looked red, dry, and glazed, and his arm and shoulder on that side were always painful and sometimes swollen.

From April 23rd to May 11th he could not lie down, any attempt to assume the recumbent posture invariably bringing on spasms; so that he was always placed on his feet at their approach, when the flexor muscles of his trunk and extremities were as tight as a bow-string.

As the disease progressed he became exceedingly pale, thin, exhausted, and ghastly-looking, the latter being heightened by the well-known "sardonic smile;" but he was of a singularly patient and uncomplaining temperament, and seldom lost hope of his ultimate recovery. After a while his legs and feet became painfully swollen—the result of position. Bathing them with warm water somewhat relieved them. What sleep he got, he got sitting up. Always after an interval, longer than common, of freedom from pain he was seized with a sharp bout of spasms.

Up to May 6th there was no marked improvement: on the other hand, the disease had not perceptibly gained on us. Sometimes he even got two or three hours free from severe pain, but then, his system having apparently become surcharged with tetanic electricity, only obtained relief by a series of violent paroxysms, one after another, in quick succession, which left him very exhausted but free again for another interval. He always flew to his mixture intuitively, declaring he could not live without it.

On the 8th and 9th of May the pains appeared permanently lessened, especially in the upper parts of his body, but they seemed to be occasionally more severe in the lower, his hamstrings being acutely painful, and the disease appeared to be passing downwards.

On May 11th (about three weeks from the commencement) he lay down in bed for the first time and opened his mouth better. The altered position relieved him greatly, and his legs soon became less swollen.

From this date he made satisfactory progress, but for many weeks was too weak to move from the house. He is now (September) following his employment as a labourer in the ordinary way.

His twenty-four hours' allowance of nourishment after the first week, during which he could swallow very little, amounted to from three to four pounds of meat made into concentrated broth; six to ten eggs; and from two to three pints of new milk; and, after the first fortnight, ten to fifteen and even twenty ounces of sound sherry, in wine whey; so that the sum total amounted to something considerable.

Between the 22nd April and 12th May, he took about 240 grains of bromide of potassium and 180 grains of chloral per diem—administered in doses of ten to fifteen grains sometimes every half-hour, but usually every two hours, occasionally at longer intervals. The total amount given in these twenty days was sixty drachms of chloral and eighty drachms of bromide of potassium, more or less.

There is probably nothing calling for any special remark in this case, which was hardly so severe as that I gave the notes of in 1876. Both were treated on the same lines; the endeavour being to wear out the violence of the paroxysms by the constant administration of what appears, to me, to be an antidote.

In such cases everything depends upon one's faith in the remedy and one's tenacity in its administration.—*Practitioner, Feb.* 1882, *p.* 99.

20.—ON URARI AS A REMEDIAL AGENT IN TETANUS.

By Dr. Leslie Maturin, Physician to Kilmainham Fever Hospital.

Within the past few years considerable progress has been effected in the treatment of nervous affections, based in some instances upon our extended knowledge of their pathology, and in others, where their anatomical origin is unknown or obscure, by the empiric exhibition of certain remedies which experience alone proves to be possessed of specific effects. The treatment of tetanus, however, is an exception. Numerous medicines have been extolled, and lately surgical procedures adopted, but the results have been in the majority of cases disappointing. The great number of proposed remedies bears in itself sad witness to their inefficacy, and having tested them all, we are still obliged to confess that science has not yet discovered any specific for this terrible malady. Certain cases, particularly chronic ones, recover, and might probably recover without any treatment, and the recoveries are attributed to the remedies adopted; but similar medication resorted to by others, though carried out in every detail, or even in the hands of its originators, has not on subsequent occasions verified their expectations.

I feel impelled to publish the annexed observations on a case of tetanus treated by the hypodermic injection of urari on the following grounds. We are almost unacquainted with the effects of urari upon the human subject. We are in complete ignorance to what extent the drug may be safely administered, or in what doses, short of toxic ones, it may exhibit a therapeutic action. Its well-known properties as an acinetic peculiarly suggests its administration in this disease. Lastly, the notorious fatality and terrible suffering induced by tetanus fully warrant its exhibition, even were the certainty of benefit disproportionate to the risk incidental to the administration of so dangerous a remedy.

And here I may remark upon the inconsistence of a legislation which is attempting to stifle science by its maudlin and arbitrary enactments. I had wished to test the action of the drug upon one of the larger animals, but not possessing the necessary qualification I elected, rather than abandon its exhibition when indicated, to test its effects in tentative doses upon the human subject, as a humane (?) legislature prevented my doing so upon an animal, doomed to die a few hours subsequently by the slaughterer's pole-axe. I do not know if by so doing I may not have laid myself open to a charge of manslaughter, had the patient succumbed to the drug or the disease, or whether, as a still further check to medical advancement, an enactment may not yet be framed for the prevention of cruelty to the human animal.

My experience of tetanus has been an extended and a melancholy one. In civil hospital practice I have seen three cases—one idiopathic, two traumatic. All three terminated fatally. They were treated with Calabar bean. During the Russo-Turkish war, 1877-78, over sixty cases of acute traumatic tetanus were treated by me and my colleagues, and every case proved fatal. It mainly supervened upon gunshot injuries, and eminently in cases of congelation of the feet, which were of terrible frequency among the unbooted Turkish soldiery during the inclemency of a Bulgarian winter, against which in the exigencies of war they were wholly unprovided. We employed most of the recognised remedies—morphia, chloral hydrate, Calabar bean, camphor, &c. I confess to having never used nicotine, as it was not available. The prolonged inhalation of chloroform seemed the best palliative; it abated the fearful sufferings and perhaps prolonged life, but was in no single instance curative. In one case I performed Chopart's amputation for tetanus supervening upon extensive destruction of the soft tissues of the foot from extreme cold. I operated early in the disease, in despair of other treatment. The spasms were not diminished in force or frequency, the patient dying of exhaustion forty-five hours subsequent to the operation, and all I can say in favour of this procedure is that it seemed to hasten the fatal issue. At times tetanus seemed to be almost epidemic. The most trifling injuries, the smallest operations were almost certain to be complicated with it—so much so that, except when imperative, we forbore performing any operation. The hygienic surroundings, ventilation excepted, were very bad, the weather was intensely cold, the tent hospitals in which the wounded lay were greatly overcrowded, and there were no alcoholic stimulants, a poor sort of native red wine excepted. A warm and equable temperature, quiet, and all the other essentials to the treatment of this disease in civil practice there were none, and these circumstances of course militated against a favourable issue. There were, however, none of the terrible outbreaks of hospital gangrene, so indicative of overcrowding and general unhealthy surroundings, which were experienced by military surgeons in the wars of the last century, carbolic and other antiseptics being very freely used.

Urari, or as it may perhaps be better known by its synonymes curara, woorara, wourali, and ticunias, appears to be a combination of vegetable drugs, used by the Indians of tropical

America for the purpose of poisoning their arrows. That portion of the compound upon which its poisonous principle evidently depends is the juice of the strychnos vine, of which two species are used, which belongs to the natural order of Loganiaceæ and the sub-family of Strichniæ (Jussieu). It is prepared for use by mixing it with pepper, coque-du-levant (Anamirta cocculus), and other acrid plants. The quality of urari is variable, but the poisonous principle is permanent even when exposed to the atmosphere for a long time. I am informed that the South American Indians, who mix the drug with a glutinous substance to cause its adherence to the points of their arrows, manufacture a large number at once, clearly indicating that the poison is not rendered inert by time, and that it retains its activity when scraped off the arrows which have been imported into this country. When taken internally urari possesses no poisonous properties, differing in this respect from the strychnos tieuté, a decoction of which, called tjettek, is used by the Javanese for a similar purpose, and in its permanency, from the juice of the upas with which the natives of Borneo poison their arrows, and which retains its active properties only for a few hours. The urarised arrows, or, more properly speaking, darts, are very slender, unfeathered, about ten inches in length, and are blown from a hollow reed about five or six feet long. They are principally used in hunting, the barbed heads being fitted into a socket in the point, this arrangement being adopted so that, in the event of the shaft being broken or shaken off by the wounded animal, the barbed point remains in the wound, the shaft being subsequently fitted with another head. The Bornean natives and the Javanese also impel their arrows from a reed. The Bosjesmans of Southern Africa lethalise their arrow heads with the secretion of the poison glands of the puff-adder and cerastes, and also with the larva of a small beetle called N'gwa by the natives, of which Livingstone gives a very graphic description. Their arrows are shot from bows, both bow and arrows being very slender. The Malayan kris or dagger is said to be poisoned by being plunged into a decomposed human body or into the stem of the pineapple. The knife is fluted by the action of acids to retain this rather uncertain poison in its grooves.

The medicinal employment of urari is not novel. So far back as 1856 M. Vella administered it at Turin in three cases of acute traumatic tetanus supervening respectively four, five, and three days upon operation for gunshot injuries. The first two cases terminated fatally; the third recovered, the spasms ceasing upon the twelfth day subsequent to its administration. He employed it locally in the form of compresses to the wounded part, of the strength of two grains to the ounce, increased to twenty grains to nearly three ounces. It produced no irritation. M. Chassaignac, in 1859, used it in a similar case, of a strength of 20 centigrammes in 120 grammes of fluid, as a lotion applied to the wound by plugs of lint renewed every two hours. He also administered it internally in the same case, ten centigrammes in one hundred grammes of liquid, one drachm every two hours. He was evidently unaware of its inertness when taken by the mouth. M. Follin at the Hôpital Necker exhibited it hypodermically without success in a case of traumatic tetanus occurring on the eighth day, ten drops of a solution of one part in one hundred, increased to twenty-four drops, being injected every half-hour. He says its administration was productive of no benefit. Gerhardt states that fifty per cent. of adult cases treated by urari recover, but he gives no statistics of the numbers treated, nor whether they were acute or chronic cases. If this be so, and if his percentage is framed upon a large number of cases, the outlook is hopeful for the employment of the drug in this country. He fixes the dose at from one-half to one-third of a grain in children. On the other hand, Demme regulates the quantity at from one-tenth to one-fifth of a grain in adults. Niemeyer recommends larger doses, ranging from one-eighth to one grain and a half. It has been proposed, to ensure a more uniform strength, to inject the active principle, curarina, but I am not aware of any recorded instance of its adoption.

Urari has been used on more than one occasion in England in the treatment of nervous affections. Mr. Alfred Simson and Dr. D. Drummond have adverted to its employment in 1878. I find a case reported in which it was injected to the amount of two-fifths of a grain for several days in succession in a case of chorea with an apparently curative result. At Manchester in 1878 it was injected in a case of hydrophobia without success. Four doses of one-fourth of a grain each were injected in ten hours. In 1876 Mr. Arthur E. Durham injected it in doses from three-hundredth to one-hundredth of a grain at intervals of two hours for seven days in a case of traumatic tetanus admitted into Guy's Hospital. The patient, a boy of fourteen years, recovered, but other treatment—morphia, eserine, and conium—was also combined, so that it is difficult to ascribe the successful result to the action of urari alone. The case was also a chronic one, not occurring until twenty days from the receipt of injury, such recoveries being by no means infrequent. I believe the drug may be used in much larger quantities than has been adopted in these cases. Children may be more, but are in all probability less, tolerant of the poison than adults; and with the former Gerhardt fixes the doses at from one-half to one-third of a grain. As may be seen by the appended case, however, a more marked diminution of the spasms followed the exhibition of small than of larger doses, the former being repeated at shorter intervals.

The action of urari is transitory, and seems to be exercised upon the motor nerves only, the sensory remaining unaffected. When pushed to its toxic extent, paralysis of the voluntary muscular system follows, and death ensues from paralysis of the respiratory muscles. I well remember assisting at an experiment, in which I sustained the action of the heart in a rabbit for nearly two hours by artificial respiration by means of a tube tied into the trachea. In tetanus the voluntary muscles are violently convulsed; they are in a true state of cinesia, resembling that artificially produced by strychnia. Is it not rational to infer that an agent which induces an opposite condition—that of acinesia—should be antagonistic to it? May it not be possible to induce a partial acinesia, to diminish the excitability of the spinal cord by urari, without pushing it to the extent of producing paralysis; or perhaps even justifiable if, by establishing that condition, we might overcome this hyper-irritability of the cord, knowing that in artificial respiration we might still hold the reins of life in our hands? Might we not, perhaps, adopt this ultra treatment as a *dernier ressort*, where a certain and terrible death stares the sufferer in the face?—*Dublin Journal of Medical Science, April* 1882, *p.* 307.

DISEASES OF THE ORGANS OF CIRCULATION.

21.—NOTES ON THE USE OF NITRO-GLYCERINE IN THE TREATMENT OF HEART-DISEASE.

By W. E. Green, Esq., Surgeon to the Isle of Wight Railway Company, Sandown, I.W.

I propose to bring before you certain results obtained by me in the treatment of heart disease with a somewhat recent remedy, viz., nitro-glycerine. This is a most potent remedy, and I believe I am not overstating its merits when I say that it deserves to rank only second to digitalis in the treatment of disease of the heart.

The action of this drug, which was first noticed by Field, of Brighton, has been experimented upon by various observers.

It is to Dr. William Murrell, however, that the medical profession is chiefly indebted for introducing it as being of great value in the treatment of certain diseases. From a consideration of the physiological action of this substance, and especially from the similarity existing between its general action and that of nitrite of amyl, as set forth by Dr. L. Brunton, Dr. Murrell was led to infer that it would probably be of service in the treatment of angina pectoris, and his result may be seen by a reference to his valuable paper in the Lancet of the earlier part of the year 1879.

I will briefly detail its mode of action and the preparation usually employed.

The solution is the form most generally used, and this is a one-per-cent. solution in spirits of wine: one minim is the usual dose to commence with, but in some cases even less may be given with advantage. It can either be taken in water or one drop may be placed upon the tongue. The solution is almost tasteless, but within three minutes of being taken it begins to exert its peculiar physiological properties. It paralyses the vaso-motor nerves, and so dilates the blood-vessels; the face flushes, the temples throb, the pulse becomes dicrotic and much quickened; in some cases the head aches most violently, but in others only a sense of fulness and pain across the forehead is experienced, which lessens with each recurring dose, until ultimately no unpleasant effect, but simply a warming sensation all over the body, is produced.

A feeling of nausea, or even sickness, is often caused by the earlier doses. The quantity may be gradually increased until 15 or 20 minims every four hours are given, but I have never found it necessary to administer such heroic doses. It is never wise to give more than one minim at first, for even this small quantity has produced most serious symptoms in certain individuals. The patient has fainted, and has become almost collapsed, but I am not aware that it has ever been followed by a fatal result. I have never myself seen these alarming symptoms, but in two cases have found unpleasant symptoms to succeed a first dose, both cases occurring in hysterical women. The medicine, in one case, was taken without my being consulted (the patient having seen it produce good effects in her father's case); in the other, I prescribed it for certain nervous symptoms, but, at that time, not having had so much experience in its use, I did not like to persevere. The physiological effect of nitro-glycerine is not so rapidly produced as is that of nitrite of amyl, but it continues from four to six, or even eight hours, after which time it is often advisable to repeat it.

I have never found it produce unpleasant effects in any case where its use was plainly indicated; and each day's experience more clearly shows the cases likely to be benefited by it.

While useful in almost all cases of heart disease, I believe those in which it will be found of the greatest benefit are, 1st, angina pectoris; and 2nd, weak, dilated, and fatty heart. In angina it prevents an attack by keeping the blood-vessels in a constantly dilated condition, and thus prevents the backward pressure of blood upon the heart, which is probably the cause of the agonising pain of angina.

In weak dilated hearts it gives relief by reducing arterial tension and thus lessening the amount of work the heart has to do; the heart, consequently, gains in power by the rest so given to it. As a rule, digitalis does not agree in these cases; but if thought necessary, it may be given with increased advantage in conjunction with this drug. In several cases of dilated heart, with small, weak, quick pulse, I have seen the beats not only increased in power, but much reduced in frequency, after taking nitro-glycerine for a few days, thus plainly showing that the heart had been relieved of much of its embarrassment, and as a consequence had gained in power. I have used this drug largely during more than two years, and each week my appreciation of its value as a remedy for this class of cases increases. There are numerous other affections in which it will prove of value, but such do not come within the scope of my present paper. Bearing in mind its physiological action, it will be easy for you to select the cases in which its use is indicated.

[The following are the notes of one of several cases related by Mr. Green, which he considers are conclusive of the value of nitro-glycerine in heart disease.]

Case.—Mrs. H., æt. 60, a spare, delicate, nervous woman, has frequently been under my care during the past three years, sometimes with severe attacks of bronchitis, at others, with neuralgic pains of an ill-defined character, but more frequently in the intercostal nerves, with extensions of pain to the left shoulder; moreover, there was always a dread of something pending, though she could not define its nature. The heart, though weak in action, was sound, although palpitations were complained of after the slightest exertion. About 2 a.m. on Nov. 11, 1879, my services were called into requisition for a most severe attack of angina pectoris. Nitrite of amyl was immediately used and a subcutaneous injection of morphia given, with the effect of freeing the patient from her attack, and inducing a sound sleep at the end of about an hour and a half. 10 a.m. Has had seven hours sound sleep and feels better, but is dreading the approach of night and the advent of another attack, thinking she could not possibly survive it. Examination of the heart shows me that there is a soft mitral systolic murmur which is probably due to strain of the valve produced by the backward pressure of the blood at the time of the attack. One-minim doses of solution of nitro-glycerine are ordered three times a day with considerable relief, but it was difficult to get the patient to take the remedy regularly on account of the unpleasant headache it caused. Nitrite of amyl to be kept in the house in case of need. The patient was so nervous and dreaded so much the recurrence of another attack, that she went far towards producing slight attacks upon the succeeding nights. On the 13th, therefore, she was ordered a draught of chloral and hyoscyamus at bedtime, with the result of inducing a night of sound sleep. She improved rapidly, and was allowed to go down stairs on the 24th inst. The exertion was apparently too great, for about 2.30 a.m., the next day, she had her most serious attack. The previous treatment relieved her, but a recurrence took place early on the morning of the 27th. Upon this day Dr. Coghill saw her, and suggested a night pill containing quiniæ sulph. grs. 3, pulv. opii, gr. ½, ext. bellad. gr. ½. This was given for two nights and then had to be discontinued, owing to certain unpleasant effects produced. Each of these nights the patient had an attack. I then gave her a draught containing chloral, cinchona, and conium, at bedtime, which draught was continued until the patient was convalescent, but the chloral and conium were gradually left out without the knowledge of the patient. During the whole of this time the solution of nitro-glycerine was continued, being increased slightly on the 27th, and to ♏ 1¼ every four hours after the 30th in conjunction with cinchona and Virginian prune. The patient steadily improved during the month of December, but was not allowed to go down stairs until her strength was much greater. She gradually lost her dread of an impending attack, and then permitted herself to leave off the night draught of cinchona. On Jan. 1, 1880, a pill of phosphorus, reduced iron, and nux vomica, was given and continued for a month, but during this time a night dose of nitro-glycerine was given. Since Feb. 1 has daily taken two doses of nitro-glycerine combined with cinchona and Virginian prune, and the result has been most gratifying. She is now feeling better than she has done for some years; from being spare, she has become stout, and the mitral bruit has entirely disappeared. During the summer of 1880 she passed easily through a severe attack of bronchitis, and at the present time is in the enjoyment of perfect health.—*Practitioner, Feb.* 1882, *p.* 103.

22.—ON THE IMMEDIATE ARREST OF BLEEDING FROM THE NOSE—THE INFLATING NASAL PLUG.

[Dr. J. Kent Spender, of Bath, in a note in the British Medical Journal, writes:]

At a meeting of the Royal Medical and Chirurgical Society, on April 12th, 1881, a distinguished surgeon exhibited "a simple instrument for plugging the nares in epistaxis. It consists of a bag of India-rubber, which, after insertion in the nares, can be inflated with air to the required size; and, exerting equable pressure, speedily checks the hemorrhage." This report, which I quote from one of the medical journals, gives the impression that the principle of the instrument was a novelty, and that no one had ever heard before of air-inflation as a means of stopping bleeding from the nose.

As a matter of fact, an instrument of an identical kind was described by Dr. Taaffe, of Brighton, in the Lancet for January 4th, 1873. I had one made after reading his letter, and it answered its purpose very fairly well for three or four years. On one occasion, however, it failed because the inflated surface did not seem large enough, and blood escaped from the posterior nares into the pharynx. Shortly afterwards, a more perfect instrument was devised by Dr. H. Cooper Rose, and an account of it was given in the British Medical Journal for January 10th, 1874. In an improved form it is again described (and represented by figure) in Mr. W. Spencer Watson's book on *Diseases of the Nose and its Accessory Cavities.* It now "consists of a gum elastic tube about five inches long, with lateral perforations near the end, and covered with thin caoutchouc membrane in the form of a spirally twisted bag for the last three or four inches of its length. To use it, the membranous bag is smoothly folded over the continued tube, and the whole being oiled (diluted glycerine is better) is passed along the floor of the nares till it reaches the pharynx. The bag is now inflated . . . and if a stop-cock is fitted, the air is kept in by turning it as soon as sufficient tension is obtained." The cavity of the twisted bag could be injected with water, if it were desired; but I have never found this necessary. When I recollect what "bleeding from the nose" was in old days, I cannot be too thankful to Dr. Rose for his simple and effective invention. To be called to an obstinate accident of this kind, especially when other medical men had failed, was enough to make one sick at heart from the possibility of adding another failure to the dreary history; and then there was the consciousness that delay might mean impaired health or even death to the victim. The victory is half won when a man is armed with an apparatus which he knows is sure to succeed; and I am now speaking of cases in which he wishes to succeed, and which are not forms of natural blood-letting to be encouraged. The object of this brief communication is to recommend Dr. Rose's instrument for (*a*) facility of introduction; (*b*) the extent and evenness of the inflated area; and (*c*) the possibility of its remaining *in situ* for thirty-six or forty-eight hours, when it may be gently removed, and the hemorrhagic nostril can be syringed with some cold astringent fluid for purposes of cleanliness and the washing away of blood *débris.*

[Mr. H. C. Howard, M.R.C.S., of Clapham, in a subsequent note to the same journal, says:]

As the inventor of the nasal plug, exhibited in April last at the Royal Medical and Chirurgical Society, I may perhaps be permitted to say a few words with reference to the article by Dr. Spender on the above subject.

When the idea first occurred to me of arresting epistaxis by means of an India-rubber bag, I was not aware of any other instrument of the kind being in existence. As soon as it was completed, and just before its exhibition at the Royal Medical and Chirurgical Society, I heard of and obtained from Paris an instrument similar to the one recommended by Dr. Spender, with this exception, that the bag is not spirally twisted round the tube, but simply tied on at both ends.

I think anyone, on comparing the two instruments, will see that they are totally different. Mine consists of an India-rubber bag, of the length of the nose, compressed in the middle, so that the two enlarged ends plug respectively the anterior and posterior nares. The narrowing in the middle is made for the purpose of allowing the pressure to be as little as possible on the inferior turbinated bone. It is passed into the nose by a probe hitched on the under side. To the anterior extremity is attached the tube (with stop-cock) by which it is inflated. (I have sent one to Dr. Spender, as I gather from his article that he has not seen one.)

I will now enumerate the advantages which I claim for my instrument. It causes less pain when *in situ*, on account of the greatest pressure being at the ends, and not in the middle, on the inferior turbinated bone. It cannot be pulled out or blown out while sneezing (as, I believe, the other can be). After insertion, and having been filled with water, it holds more water by about one-fourth, proving that the cavity of the nose is more occupied, especially at the ends, where the pressure is needed, and therefore better plugged. I have the authority of Mr. T. Holmes, the "distinguished surgeon" referred to, for saying that he is of opinion my invention is superior in construction to any he has seen. It is made by Messrs. Walters and Co., of Lambeth Palace Road, and is called the "Inflating Nasal Plug."—*British Medical Journal, Nov.* 12, *and Dec.* 3, *pp.* 776, 896.

DISEASES OF THE ORGANS OF RESPIRATION.

23.—CONCLUSIONS CONCERNING THE GENERAL PATHOLOGY OF TUBERCLE.

By Sidney Coupland, M.D., F.R.C.P., Physician to and Lecturer on Pathological Anatomy, Middlesex Hospital.

1. Tuberculosis is an infective disease to which man and the higher animals are liable.

2. It is characterised anatomically by the formation of minute

nodules or "granulations," composed of elements like those met with in granulation-tissue, the result of simple reparative inflammation.

3. These nodules, or elementary or primary "tubercles," may occur in an isolated manner, or, by their confluence, may form larger or smaller conglomerate masses.

4. The typical structure of each fully formed primary nodule consists in (*a*) a collection of lymphoid round cells, enclosed in a delicate fibrillar meshwork or stroma; (*b*) in an internal zone, more or less evident, of larger nucleated epithelioid cells; and (*c*) a central multi-nucleated or giant cell.

5. These "tubercles" arise apparently in connection with the lymphatic tissue that pervades the body. No region is exempt from them. They may occur in the substance of organs, in the bones and muscles, in serous membranes, as the pia-arachnoid, pleura, pericardium, and peritoneum; in synovial membranes; in mucous membranes (arising in the submucous stratum), as in the mouth, pharynx, larynx, trachea, bronchi, intestines, and genito-urinary tract.

6. Being ill supplied with blood-vessels, they can only attain a certain size, and then perish. The central cells degenerate first, because they are the farthest removed from the nutrient blood stream, and mutual pressure due to their increasing growth hampers their vital activity. They become fattily degenerated, soft, opaque, caseous, forming "yellow" tubercles, which, when isolated, are larger and manifestly of older formation than the miliary translucent grey granules. Where such tubercles are confluent, larger and more irregular caseous masses are formed. Caseation may pass into cretification. On the other hand, there is no doubt that occasionally the tubercular nodules take on a fibroid change, passing from the stage of "granulation-tissue" to one resembling "cicatricial tissue."

7. Almost invariably there occurs, in the vicinity of the tubercular formation, some reactive inflammation. This may be protective by ultimately leading to encapsulation by fibrous tissue of the caseated tubercular focus; or, as more frequently happens, it aids in the disintegration of the surrounding tissues, and leads, with the necrosis of the tubercles themselves, to destructive ulceration.

8. Individuals who are prone to the development of tubercle are called "tubercular." The disposition may be inherited. Probably what we recognise as "struma" or "scrofula" is only one form of this: a tendency to tuberculosis of lymphatic glands especially; just as in phthisical subjects we have a tendency to pulmonary tuberculosis.

9. The tubercular manifestation is, in the majority of cases,

at first local, *i.e.*, limited to one organ or tissue. It may remain so limited throughout life—may not even endanger life—or may lead to death by the local destruction to which it gives rise. On the other hand, it may be more or less widely diffused throughout the body of the same individual. This diffusion may be due sometimes to the simultaneous development of tuberculosis in many parts. More frequently, it is due to secondary dissemination, by a process of infection.

10. This dissemination takes place, as in cancer, in two ways: viz., by direct extension, or infection of neighbouring tissues by contiguity; and by general distribution of the tubercular virus through the medium of the blood-system (including lymphatics).

11. The tubercular virus seems to be most potent, or, at any rate, to retain its potency, *i.e*, its infective property, in the caseous state.

12. Examples of the local extension of tubercle, or of propagation by contiguous infection are seen: (1) in the development of peritoneal tubercle from intestinal; (2) in the spreading of tubercle from one part of an organ (*e.g.*, lung) to another part; (3) in extension from lung to pleura; (4) in bronchial, laryngeal, and intestinal ulceration excited by the passage over their mucous membrane of material expectorated from a phthisical lung; (5) in tuberculosis of bladder and vesiculæ seminales following upon renal or testicular tubercle, etc. The mode of its local extension approximates tubercle to the neoplasmata, viz., by its elements exciting in the tissue they infect changes leading to the formation of cell-masses resembling the primary focus.

13. The generalisation of tubercle is shown in the disease known as acute miliary tuberculosis, which is characterised by an eruption of miliary granulations in diverse organs and tissues. Its mode of occurrence may be (as above) compared to the general dissemination of secondary cancer, or, perhaps with equal truth, to the metastatic suppuration of pyæmia. With few exceptions, it appears to necessitate a primary tubercular focus to give rise to it. It is believed that the infective virus, whatever it be, enters the blood-stream at this local focus, and is thence widely disseminated, the resulting growths being for the most part miliary, grey, and translucent; life not, as a rule, being prolonged for a sufficient length of time after the occurrence of the generalisation to permit of the growths becoming confluent or caseous. As the membranes of the brain are generally involved in this widespread infection, death occurs early.

14. Lastly, tuberculosis is inoculable. In this respect, it resembles pyæmia, and differs from the cancers; for there is

reason to think that it may be and is communicated from one human being to another, *e.g.*, from husband to wife, and *vice versâ;* and that it can be inoculated in animals from man (artificial tubercle). There is, further, a possibility, based on certain peculiar morphological resemblances of the formations, that bovine tuberculosis is communicable to man.

15. If the foregoing data be true, it follows that tuberculosis is an infective disease, probably due to the presence of a virus, which gives rise to the development of peculiar tissue-formations, capable of localised or general propagation in the body, and characterised mainly by their tendency to early disintegration.

16. Until the nature of the virus is known, it is impossible to formulate data concerning the conditions under which the disease arises in subjects free from inherited taint.—*British Medical Journal, Feb.* 11, 1882, *p.* 186.

24.—DR. KOCH ON A NEW TUBERCULAR GERM.

By the Editor of the Medical Times and Gazette.

Dr. Robert Koch, who is favourably known as the author of an essay on "Wound Infection," and who went to Berlin a year or two ago in order to direct the pathological inquiries of the Imperial Board of Health, has arrived, in the exercise of his official duties, at the old and obdurate problem of the etiology of tuberculosis. He has attacked the tubercle question in a series of elaborate researches, of which he communicated the sum and substance to one of the Berlin societies—the Physiological—on March 24th last, in a lecture occupying the greater part of the space of the *Berliner Klinische Wochenschrift* of April 10. Dr. Koch's motive for taking up the tuberculosis question hardly required to be stated. That terrible disease causes one-seventh of the whole mortality in our latitudes, and, if we exclude children and old persons, it causes the death of one-third of the population. A disease that removes so many in the flower of their age may well be a care to the Imperial Board of Health; and to those old-standing questions which have hitherto baffled the penetration of the wisest of our profession, Dr. Koch has returned an answer that is singularly free from ambiguity. Tuberculosis (including all the varieties of consumption in man, the corresponding disease in the ape, and the *Perlsucht* of cattle) is a parasitic disease of the internal organs; the parasite is a bacillus, which may be distinguished from all other bacilli by its remarkable behaviour towards the colouring reagent "vesuvin." It is not the actively moving tubercle-organism of Klebs, for it is stationary; it is not the spherical micrococcus of Schüller nor of Toussaint, for it is rod-shaped; it is not the rod-shaped organism which Aufrecht found (along with two sorts of micrococci), for that was only twice as long as it was broad, whereas the length of the new bacillus exceeds its breadth five times. The tubercle-bacillus of Koch is a slender, rod-shaped, motionless body, in length one-quarter or one-half, or the whole diameter of a red blood-corpuscle, with or without oval "spores" clinging to it, and approximating in morphological features, on the whole, most closely to the "bacillus of leprosy," although the latter is a little more slender, as well as pointed at the ends. How does Dr. Koch arrive at this remarkable conclusion, so pregnant with importance to the well-being of mankind?

Dr. Koch does not seek to disguise the fact that he approached tuberculosis purely from the germ-theory side; he does not profess to have taken the entire general pathology of tubercle within the purview of his study. He frankly set to work to look for a minute organism in tuberculous formations of various kinds, and, after a good deal of disappointment, he at length stumbled on the suitable procedure for bringing the existence of the organism to light. The tuberculous substance was either spread out upon a cover-glass, dried, and exposed to heat, or a piece of a tuberculous organ was placed in alcohol and afterwards cut into fine sections. A particular solution of methylene-blue was made, a weak solution of potash being added; the cover-glass coated with tuberculous matter (or a section of the organ) was then placed in the solution for twenty to twenty-four hours, but half an hour sufficed if the solution were warmed in a water-bath up to 40° C. The cover-glass, which comes out a deep blue, is then treated with a concentrated watery solution of "vesuvin" for one or two minutes, and is afterwards washed with distilled water. The blue of the methylene has visibly changed to brown; under the microscope all the amorphous detritus and fragments of tissue spread out on the glass are brown, *but the tubercle-bacteria remain blue.*

The bacillus of leprosy is the only organism that shares with the bacillus of tubercle the remarkable faculty of resisting the brown staining of "vesuvin"; and even the leprosy-bacillus will take on a certain colouring (devised by Weigert) which the bacillus of tubercle resists. Dr. Koch had therefore a means of picking out the bacillus of tubercle from among the most puzzling surroundings. Curiously enough, it was in the interior of giant-cells—still destined, evidently, to play a great *rôle* in tubercle—that the bacillus was oftenest found, and in slowly progressing cases of tuberculosis the bacillus occurred nowhere but in the substance of those multinuclear elements; they are found as small blue rods, sometimes only one, sometimes several, up to twenty, in the midst of a prevailing field of brown. Not every group of giant-cells, and not every giant-cell in a group, would show the bacilli; many giant-cells are quite free of them, but these are the old cells which had once held bacilli also, and had somehow got rid of them. The bacilli are enclosed by the giant-cell probably as foreign bodies, just as those multinuclear masses are found to enclose vegetable fibres and the ova of strongylus. When the tuberculous eruption "has passed its highest point," the bacilli become fewer, and may even disappear altogether. They are extremely numerous in cavities in the lung; "the well-known small caseous friable masses in the interior of cavities consist almost entirely of bacilli," which are often covered with spores. In the larger kind of cavities they are mixed with other bacteria; but, under the above-described staining procedure, *the tubercle-bacteria alone are blue, while all the others are brown.* It is right to state, however, that the bacillus of tubercle may sometimes be observed without the blue staining. More than two hundred guinea-pigs and rabbits were successfully inoculated with various kinds of tubercle in the usual way; the bacilli were not missed in the tuberculous formations of a single one of these animals. Such being the uniformity of their occurrence, why, asks Dr. Koch, have they not been seen before? The answer is that they are very minute, and often very few, and that only the particular colour-test suffices to detect their presence.

Thus far, Dr. Koch has proved that a bacillus co-exists with tubercle, its most universal *locale* being the interior of giant-cells. Next follows the proof that the bacillus is not a mere epiphenomenon of tubercle, but its cause. This is accomplished by the now familiar method of obtaining the pure bacillus, free from all suspicious animal particles clinging to it, by means of successive "cultivations." We at length reach the bacillus pure and simple, carrying with it nothing but its own inherent qualities, be they vicious or be they neutral; a sufficient amount of bacillus is introduced into an animal, and the effect is watched. Klebs, Schüller, Toussaint, and Aufrecht have each cultivated a bacterium from tuberculous matter, and have set up tuberculosis by injecting it; but they cultivated the organism in an albuminous fluid, which became turbid in about three days after the piece of tuberculous substance (containing the tubercle-bacteria) had been deposited in it.

Dr. Koch, on the other hand, found that his bacillus was "troublesome" to cultivate in fluid, and he did not persevere with that method, obvious though its advantages are. He took the blood-plasma of the ox or sheep in a test-tube, and after carefully sterilising it by repeated applications of heat, he boiled it to a coagulum, at the same time inclining the test-tube so that the coagulum might cover a considerable surface.

The clot was a transparent, yellowish, firm jelly, and it was on that "nutrient soil" that he proposed to "grow" the tubercle-bacillus without the intervention of moisture. How did he gain his end? He took from the dead body—usually from the lung of the ape or of man—a piece of tuberculous substance, using great care to have his instruments disinfected; the fragment was then washed several times in corrosive sublimate, the outer layers were removed, and from the interior there was taken a portion "into which it was to be expected that no bacteria of putrefaction had penetrated." The cotton-wool plug was then removed for a moment from the test-tube containing the layer of coagulated plasma; the piece of tubercle was introduced, broken up, and scattered over the surface; the plug was then replaced, and the test-tube deposited in an oven kept at a uniform temperature of 37° to 38° Cent. During the first week nothing happened; if any activity did show itself, it was a sign that the bacteria of putrefaction were present, and Dr. Koch—we think mistakenly—did not persevere with that experiment. Usually after the tenth day there became visible to the naked eye, on the dry surface of the coagulum, "a number of very small points or dry-looking scales, which surrounded the pieces of tubercle that had been laid out, in circuits more or less wide, according to the extent of breaking up and dispersion of the tubercle fragments at the time when they were sown." The sentence is an important one, and we regret that we cannot make it look more explicit in the translation. Were those dry scales other than some of the scattered fragments of tubercle? The point is not discussed by Dr. Koch, and yet it seems to require elucidation. However, the dry scales (or crusts, as they sometimes were) are taken to be colonies of the bacillus. After a few weeks' exposure to dry heat, the scales or crusts cease to enlarge. The cotton-wool plug is then withdrawn, and they are transferred on glowing platinum wire to another test-tube similarly prepared; they are broken up and scattered over the surface, and the plug replaced. "After an equal interval, dry scaly masses again appear, become confluent, and cover more or less of the coagulum surface, according as the seed was widely scattered;" and so, from test-tube to test-tube, is the "culture" continued for as many as a dozen times,] and through a period extending up to one hundred and fifty days. It is not stated wherein the dry scales and crusts of the later cultivations differed in quality from those of the earlier. With those bacillus-cultures (dry scales or crusts), numerous animals were inoculated, either under the skin, or into the peritoneal cavity, or into the anterior chamber of the eye, or into a vein. In upwards of thirty of the cases the substance was taken (and cultivated) from the tuberculous lung of the ape; pieces of human phthisical lung furnished seventeen of the cultures; and the "pearl disease" of the ox furnished the material for ten. Without a single exception, all the inoculated animals (chiefly rabbits and guinea-pigs) acquired tuberculosis of the lungs, liver, spleen, and other organs; and the tubercles had the structure of the original tubercle, including giant-cells, which latter contained bacilli.

Dr. Koch claims that these remarkable results, which have long been known to follow inoculation with even minute quantities of tuberculous substance, are due to the introduction of the bacillus *per se.* Perhaps it is that we have not the full details before us, but we confess to a certain difficulty about the crusted or scaly pieces of substance with which he obtained those results. What was their relation to the fragments of tuberculous lung scattered over the prepared surface? What became of those fragments? We venture to think that these questions require an answer before we admit that the pure bacillus, and nothing but the bacillus, was the means of producing the tuberculosis. The only novelty in Dr. Koch's practical suggestions relates to the sputum of phthisical patients. Even the dried sputum will produce tubercle when inoculated; and sufficient care has not hitherto been taken to reduce the expectorations of the phthisical to a condition in which they can do no harm.—*Medical Times and Gazette, April* 22, 1882, *p.* 411.

25.—ON THE USE OF AGARICUS IN THE TREATMENT OF NIGHT-SWEATING IN PHTHISIS.

By John M. Young, M.A., M.B., C.M., Resident Physician, Western Infirmary, Glasgow.

The drug agaricus, whose influence in checking night sweating was lately brought into notice in this country by Dr. Wolfenden, of Charing Cross Hospital, has been used pretty extensively during the last few weeks in Dr. Gairdner's wards in the Western Infirmary, with results of an extremely satisfactory character.

The drug, derived from the common toadstool, is a light brown bulky insoluble powder of very nauseous taste, and on this account cannot well be administered as such. The plan adopted in the wards at first was to give it, imitating Dr. Wolfenden's practice, in doses of from ten to thirty grains made into an electuary with honey. Although its efficacy against night sweating was very marked when given in this combination, considerable nausea or even sickness was often found to be the result; so that a tincture, of the strength of ten grains to the drachm, prepared by the hospital dispenser, Mr. Hay, was at length substituted, and found quite satisfactory, a proportionately smaller quantity of the drug in this form being required to produce equal effects. A few grains of the principle, which perhaps may be called agaricine, crystallising in long needles like those of strychnine and other alkaloids, were afterwards extracted by Mr. Hay, and administered in pill form, each pill containing 1-12th of a grain, and one or two forming a dose according to circumstances. This is perhaps the most acceptable form in which to administer the drug, and the therapeutic effects obtained have been practically the same.

Without taking up space by citing cases, these therapeutic effects may be classified as follows:—

1. Night sweating becomes proportionately lessened according to the amount of the drug administered; and if sufficient doses be given, becomes effectually checked or prevented, according to the time of administration.

This was observed to be the absolute rule in all moderate cases of phthisical night sweating where it was tried, and in one case, where the sweating was colliquative, agaricus brought the sweating within moderate limits; though probably, if sufficient doses had been given, it might have cut it completely short. In other cases of sweating not dependent on phthisis the drug was found to work well; indeed, its most notable effect was observed in the case of a patient who had had sweating of the right side of his body and right leg persistently at night for eight months, and in which a dose of ten grains stopped the symptom for a night almost at once.

Indeed, in the quickness of its action, agaricus resembles atropin. Like atropin, too, the effect is not permanent. In some cases, during the second night, sweating was found to be reduced below its average amount, or even entirely absent; but in the majority of cases, unless the drug was repeated, sweating returned as intensely as ever, and repetition of the drug during a considerable period does not appear to lead to any permanent result. Observation rather points to the necessity of gradually increasing the dose under such circumstances.

2. The effect of the drug against sweating is not more marked than its effect in promoting sound sleep, and relieving troublesome cough, especially of phthisis. This is the most notable fact to the patients themselves, a gradually increasing feeling of drowsiness following its administration in most cases, and this I can confirm from personal experience. In one case in particular, characterised by extremely troublesome and painful coughing during the night, to combat which numerous remedies had been tried with little or no effect, after the first large dose of tincture of agaricus this symptom became entirely subdued, and the patient had sound sleep. Her cough

has since been effectually kept under by the remedy. In another case of severe spasmodic cough, of nervous origin, agaricus gave considerable relief when first administered.

The antihydrotic and soporific effects of the drug are probably distinct from one another, and not to be related as cause to effect, since, in some of the cases observed, sweating was always complained of as occurring *during sleep*, the body being covered with perspiration on awaking. Cessation of cough may, however, be the result of the soundness of sleep.

3. Strict investigation seemed to show that the drug has considerable influence in reducing the temperature, in cases of phthisis at least, where the fever is unstable, as much as 2 degrees of a fall having been observed within half-an-hour after administration of 1-12th of a grain of agricine. Such a marked result was not always obtained by repetition of the experiment in different cases; but the fall observed in this way averaged 1°.4, and had its expression in a relieved feeling on the part of the patient.

Along with the effect on temperature, it may be noted that the rate of the pulse becomes usually reduced to a corresponding degree, but this effect has not been invariably noticed.

Reasoning from the analogy of the effect of nitrite of amyl on the blood pressure, I compared in five or six cases sphygmographic tracings immediately before and half-an-hour to an hour after the administration of the drug. I was not surprised, however, knowing the difficulty of registering fine differences in pressure by means of a sphygmograph, to find in most cases no discernible difference. In one case nevertheless (probably one of more than usual susceptibility to the remedy), several trials showed that the tracings of the pulse which, when uninfluenced, had a decided leaning towards the characters of dicrotism, after the administration of twenty grains, came to assume characters pointing to greater tension, the aortic notch diminishing or entirely disappearing. The rate of the pulse at the same time fell from six to twenty beats. These observations may have some bearing on the rationale of the effects of the drug, but they are far from being free from the chance of fallacy.

Since the administration of the tincture was begun, complaints of sickness were only heard in *one* case, in which the drug had been given over a considerable period. Diarrhœa was only occasioned once, and that in a case where this symptom recurred at intervals independently of the drug. Its laxative action, indeed, was noticed by some patients as very satisfactory. Dr. Wolfenden found that such diarrhœa could be averted by combination with one or two grains of Dover's powder. No other bad effects have been observed.

There can be no doubt, therefore, that this remedy is a very efficient one, specially against some of the most prominent subjective symptoms of phthisis.

Good results have also followed its use in some of the other wards in the Western Infirmary. A sufficiency of the principle of the drug has not yet been obtained to make up a solution for hypodermic injection. Probably the effects following on this method of administration will be even more decided.

No comparative trial of its merits as against other medicines with similar effects was made, the object sought for having been rather to confirm it as a drug at once innocuous and thoroughly to be depended upon against night sweating when administered by the mouth.

Dr. Wolfenden's remarks on this point may be quoted in conclusion:—"Having made extensive trial of those remedies commonly in vogue for checking the debilitating night sweats of phthisis, I have found many sadly wanting, and only *one* —viz., atropin—that yielded really excellent results. Given in 1-70th of a grain doses it rarely fails. But atropin is a drug of great power, and capable of producing great physiological effects; and, though accidents are very rare, such an accident as an overdose might easily occur, and dispensing is not always faultless. . . . . . The complete success of agaricus has led me to consider it of value equal to atropin, and the superior to atropin from the fact that it is quite innocuous. . . . . I believe it to be a preparation worthy of extensive trial, and of such equal efficacy with atropin that it may supplant it." *Glasgow Medical Journal, March* 1882, *p*. 176.

26.—ON THE PREMONITORY STAGE OF PHTHISIS AND ITS IMPORTANCE IN RELATION TO TREATMENT.

By John S. Main, M.D., Manchester.

"Coming events cast their shadows before," so wrote Campbell. Since then the saying has ever existed; and perhaps will only cease to exist with time itself. Applied to medicine as well as to every day life, it is also very true; in fact, here it might be extended *ad libitum.*

The fevers have their premonitory stage, or stage of incubation. Pneumonia, pleurisy, rheumatism, and Bright's disease have theirs; and time would fail me to speak of many others. That a premonitory stage exists in phthisis pulmonalis, as well as in some other forms of disease due to the tubercular diathesis, is a fact about which I think few at the present day are altogether sceptical. But before proceeding farther, I would mention to what form of phthisis my remarks are meant to refer.

Phthisis may, for practical purposes, I think, be conveniently divided into two forms.

Firstly. Those forms due to causes acting directly on the lung tissue. Under this class I would enrol three distinct forms, viz.:—

(*a*.) That owing to repeated irritation of the air passages—the result of frequent colds.

(*b*.) That due to the direct irritation of foreign particles—the so-called steel-grinder's phthisis, miller's phthisis, collier's phthisis, in short, "fibroid phthisis."

(*c*.) That sometimes following on the direct inflammation of the lung substance itself—the so-called "pneumonic phthisis."

Secondly, And it is to this form that my remarks apply. That form of phthisis due to causes acting constitutionally, and through some default in the system in general.

In this form again, the disease may be primarily set up in the lung, or it may be secondarily set up there owing to infection carried from some previously affected part.

As to the "etiology" of phthisis, it is not my intention here to enter. Suffice it to say, that underlying all these forms—with the exception of the form called "fibroid"—there is the same tendency of the tissues, under certain circumstances, to develop tubercle; in fact, this tendency (perhaps not strictly definable) is the foundation upon which all forms of *true* phthisis are built. Should a person so disposed be placed under marked predisposing causes, the tendency to develop tubercle, even though only slight, may be brought out; and again, should a person even markedly tending this way be placed in circumstances in every respect favourable, he may pass even a long life without developing any active signs of tubercle whatsoever. In short, if the resisting power of the system—the so-called "vis a tergo"—be for any reason reduced below *par*, then the constitution is, as it were, off its guard, and being so, such persons may be attacked at a disadvantage—in other words, tubercle may develop itself; but if the same "vis a tergo" be kept in advance, and superior to the other, then the one force may be sufficient to counteract the other, and perhaps even suppress it through life. All constitutional treatment points in this direction—viz., the raising of the resisting force of the system. Should the disease be taken in the premonitory stage, we may be capable thus of checking it, or if once set agoing, arresting it, and for the time being at least, apparently curing it.

So far as I have been able to judge (and I have given special attention to this subject), it is my positive opinion that the premonitory stage in phthisis is, as a rule, as well marked as it is in another form of tubercular disease—viz., tubercular meningitis, in which this stage is rarely wanting. What the symptoms are it would be rather difficult to state, as they vary much, and are by no means well defined. Still they are such as to leave no doubt on one's mind that signs of tubercular deposit may soon appear. In some cases I have even examined the patient's chest carefully with the expectation of finding such there *then*. I by no means mean boasting when I say that on several occasions my preconceived opinions on this point have proved correct; and that at the time I have entered them in my case book as "latent phthisis."

In one case I succeeded in making out the following points from a patient:—That he did not feel well, nothing marked, but more a general feeling of *malaise;* that he sometimes had a coughing attack after meals, for which he knew no cause. *Apropos* of this, I would remark that this is a not uncommon symptom in early phthisis, often very valuable for purposes of diagnosis, and depending, no doubt, on the sympathy between the lungs and the stomach, owing to both these organs being supplied by branches from the pneumogastric nerve, the action itself being reflex. He thought also he was liable to sweat more than he should be, and that his appetite was not so good as usual. This combination I thought very suspicious indeed, and at the same time not a bad synopsis of such signs as we may expect at a very early stage. I examined this patient's chest most carefully, but was rather disappointed—medically speaking—that I could not make out even one symptom to lead me to think there was any tubercular deposit there at that time. I warned my patient, however, just the same as if there had been tubercle present. Unfortunately, I saw him no more for thirteen weeks, when he again returned. On inquiry, I found that after taking the first bottle of medicine, he desisted, and thought he was quite well again. At this visit the former symptoms had returned in a worse form, and on examining his chest I then found dulness, &c., at the right apex. From the time of his second visit I had him under my care for a few months. At the end of that time the dulness and other signs were almost gone. The improvement during these few months was most marked; and I have no doubt from the experience I have had in other cases, that had I been able to have this patient under my care for a few months *at the outset,* I might have prevented the disease from appearing altogether. When me meet with cases in the very early stages of deposition I have found active local and constitutional treatment to be of great advantage. The local treatment I prefer is that by a succession of small blisters, or by keeping up a continuous irritation by the rubbing over the affected part of the ointment of tartarated antimony, *properly prepared.* The counter-irritation stimulates the absorbents and so leads with other means to the removal of the deposit. The constitutional treatment I prefer is that of a combination of tincture of the perchloride of iron and quinine, given after meals with cod liver oil.

As treatment early applied cannot but be most beneficial in all cases of phthisis, it would be well, I think, to bear in mind that a premonitory stage of the disease *does* exist, and to treat all suspicious cases as if the disease itself actually *did* exist. Prevention at all times is better than cure, and surely this is true of phthisis, for when once it is set up, "behold, what a great fire a very little matter kindleth!" Taken in this way I am sure phthisis is not such an incurable disease as many think it to be. Even in the early stage of deposition I don't think phthisis is *by any means* an incurable disease. The great difficulty, no doubt, is to get our patients to consult us in time, losing thus the very best opportunity both for themselves and us. Still, such opportunities do occur; and so we may have it in our power at times to save valuable lives. In families especially, where we have known phthisis to exist in one case, we can be on our guard for it in others.

Should these remarks be the means of bringing this stage of phthisis more prominently before any of my professional brethren than hitherto, I shall feel amply rewarded.—*Glasgow Medical Journal, March* 1882, *p.* 184.

27.—ON THE USE OF LINIMENTUM CROTONIS IN THE TREATMENT OF ACUTE BRONCHITIS.

By R. Park, M.D., Medical Officer of 3rd and 4th Districts, City Parish, &c., Glasgow.

Some recent experiences lead me to think that for a very common affection like acute bronchitis lines of practice are unnecessarily divergent, and often fail of the object in view. Having had several attacks of bronchitis in my own person and family, and being constantly called to treat patients who could not possibly lay up, and paupers, whom it is always necessary to get well as quickly as possible, I have gradually evolved a line of treatment which I may say has been invariably successful during the last five years, and which I now desire to unfold.

As soon as the patient comes under treatment, the chest, in whole or in part, is to be rubbed with two or three drachms of lin. crotonis. If the patient is seen at the very earliest stage, when there is only a slight roughness and pain behind the sternum, then it will be enough to have it rubbed into the episternal hollow, along the clavicles, and down the front of the sternum as far as the ensiform cartilage. Otherwise the whole front of the chest, the sides thereof under the armpits, and the back between the shoulders, should be rubbed. The best manner of application is by means of a tag of cotton-wool saturated with the liniment and rubbed till it is dried up. Care should be taken, and the patient warned, not to let the liniment *run* down the loins and abdomen, which it is apt to do. Also the applying hand should be carefully washed *immediately* afterwards. The application *at once* relieves the patient, and this relief is maintained and increased by having the surface rubbed covered over with a thin layer of absorbent cotton-wool, properly retained to prevent shifting. For this purpose an old chamois leather vest answers well.

From repeated personal experience I know that the liniment applied in this way does not produce pain. Sometimes, after twenty-four hours or so, there is a feeling of tenderness or soreness; but, if the cotton-wool be not disturbed, and violent rubbing be not resorted to by the patient, this soreness is evanescent, and succeeded by an itching—sometimes very considerable—which remains for three or four days. It may be relieved by smearing the eruption over with fresh butter, lard, or vaseline *under the cotton-wool*, care being taken, however, not to disturb the cotton where it has become adherent with discharge.

It is this persistence and continuity of effect which makes the application of such signal value, more especially for children and infants. So long as the irritation lasts, so long does the derivative action continue. But it has another great advantage, viz., that it enables the patient to go about his business. There is an impression abroad that it is dangerous for a patient to expose himself out of doors with an eruption such as that of lin. crotonis out upon him. Such an impression is quite erroneous. Of course I am writing in general terms. Social position, idiosyncrasy, complications, and direction of prevalent wind have all to be taken into account and allowed for. There is another prejudice against applying it to infants. I can only say that I have had it applied to scores of infants under twelve months, and have never once regretted the practice. On the other hand, I can remember many a time regretting having ordered a poultice under similar circumstances. Only last week I was called to see an infant of seven weeks, which was suffering from orthopnœa of a spasmodic kind, with retraction of abdomen during inspiration, and slight retraction of intercostal spaces. The infant, in common with some other members of the family, had caught cold with sore throat about thirty-six hours before, but nothing much was thought about it at the time. The infant had passed a restless night, and in the early morning a hot poultice was put upon its breast, with the result of soothing it for a little; but during the hour prior to my visit it had become rapidly worse. The diagnosis lay between croup and acute bronchitis. Believing the latter to be right I had the whole thoracic parietes rubbed at once with the liniment, and prescribed a diaphoretic expectorant and sedative mixture, with the effect of placing the child out of all danger in less than four hours, and preventing the long chronic bronchial catarrh which is the usual relic of acute bronchitis when treated by poulticing, emetics, and nauseants, when the patient lives. As I write now, however, on the eighth day of the attack (October 20, 1881) there still remain numerous wheezy râles, and some inspiratory difficulty, attended at times by very severe cough. In all other respects the infant is well. For the relief of the cough I have prescribed this mixture, which has answered well:—℞. Acidi hydrobromici, 80♏; vini ipecac. 100♏; tinct. belladonnæ, 40♏; acidi hydrocyan. dil. 4♏; syr. scillæ, ʒ jss; glycerini, q.s. ad. 2℥. Ft. Mist. Cujus cap. coch. min. j. 2 dis. horis.

When a poultice is applied to the chest in the early stage of bronchitis the inflammation rapidly spreads into the minute bronchioli; when applied in the later stages it weakens the patient needlessly, and increases catarrhal exudations into bronchioli and air-cells, frequently eventuating in œdema of lung tissue; and, if there should happen to be a scrofulous or tuberculous tendency, laying the foundation for a more or less acute phthisis. I am convinced that I have seen more than one life sacrificed to this treatment, and I recall one instance of a very dear friend whose life, by professional etiquette, was barely rescued from sacrifice to it.—*Practitioner, March* 1882, *p.* 170.

28.—ON DISEASES OF THE CHEST CAVITY REQUIRING SURGICAL TREATMENT.

By JOHN MARSHALL, F.R.S., Prof. of Surgery at University College, and Senior Surgeon to the Hospital; Consulting Surgeon to the Hospital for Consumption, Brompton.

Spontaneous perforation of an empyema on to the surface of the chest (for the sero-fibrinous exudations do not perforate), or, as it is called, "empyema necessitatis," occurs very frequently at one given anatomical spot. Of the first eighteen cases of this complication or termination, which I met with in printed records in which the place of perforation is alluded to, one only occurred in the sixth intercostal space; the rest were presumably in, or not lower than, the fifth space; and I can call to mind six cases in my own experience. The older writers, who saw more instances of perforation than ourselves, speak of the thoracic fistula as being commonly in the region beneath the nipple—according to Billroth, "unter der Brustwarze"; others describe it as being in the mammary or pectoral fold; and in many instances, including two in children, the spot is accurately defined as being in the fifth space. It appears, however, that in children the perforation often takes place higher up, even as high as the second interspace. As to the fifth interspace, the arrangement, in ordinary cases, of the adjacent muscles is as follows. The external intercostal muscle terminates, as usual, near the costal cartilages, to the outer side of the *spot* in question, at and beyond which, towards the sternum, only a thin fascia covers the internal intercostal muscle; the lesser pectoral, which ascends from the fifth rib, is of course altogether above the fifth space; the outer border of the rectus muscle of the abdomen passes up to the fifth cartilage, and limits the spot on its inner side; superficially to these parts the thick bundle of the great pectoral, which springs from the sixth cartilage, crosses above this spot, although a few thin fibres which arise from the sixth rib, and others which blend with the external oblique muscle of the abdomen or with the sheath of the rectus, may pass over it; or the slender upper fasciculus of the oblique muscle itself may cover it. Here, in fact, is a part of the chest wall, below the fifth rib and beneath the nipple, relatively unprotected by the adjacent muscles. The internal intercostal muscle, the thin intercostal fascia, and the weakest portions of the great pectoral and the external oblique, together with the common fascia, are, besides the skin, the only coverings of the pleural sac in this situation. It is a very weak part of the pleural boundary, and this is a valid reason why special bulging and spontaneous perforation should occur preferentially in this region—that is, in the infra-mammillary line in the fifth interspace. It may also be remarked that this spot corresponds nearly with the middle of the pleural cavity when that is much distended. An empyema may be regarded as a great pleural abscess, and, as we know, abscesses often point opposite their centre. It is, further, true that the fifth intercostal space is wider than those below it, and its limiting ribs being held to the sternum give firmness to its borders, conditions which may help the thinness of the chest wall here in determining the place of perforation. The spaces above are still wider and equally well supported by the ribs, and there is an interfascicular part of the great pectoral over the second space, which is exceptionally thin, and it is here that perforation is said often to take place in children.

In speaking of the pathological conditions and complications attendant upon pleuritic effusions, I have purposely directed your attention, first, to the character of the effused fluid, for on this their appropriate surgical treatment will be found to depend. There are two essentially different modes of such treatment, which I would term the *close* and the *open* treatment. The former consists either of aspiration, or of paracentesis with a fine trocar and cannula, followed by immediate closure of the puncture; the latter, either of paracentesis with a large trocar and cannula, or of incision into the pleura, followed by open drainage.

Now, from the evidence already accumulated on this subject, it appears to me that two general propositions may be held as established—namely, (1) that all sero-fibrinous pleuritic effusions may be treated hopefully by the close method; and (2) that sero-purulent and purulent effusions cannot be so treated hopefully, but require the adoption of the open method. If, therefore, the well-known signs of the presence of fluid being evident, the diagnosis as to the character of the effusion be otherwise obscure, a small exploring syringe armed with a fine, surgically clean—that is, a properly disinfected—aspirating needle, about two inches in length, may be introduced to determine this point, and the proper mode of treatment will then be plain. If the fluid be sero-fibrinous, the time at which the operation should be done will be regulated by the quantity of the fluid, the date of its effusion into the pleura, and of course the urgency of the symptoms produced by it. In children, for example, pleurisy is usually very acute; often in a few hours the child is panting for breath, and in a few days the chest is full of fluid; moreover, the effusion in children is very frequently purulent, either rapidly becoming so, or being so from the beginning. In children therefore the rule is that prompt relief should be given. In young persons about the age of puberty, the fluid also forms quickly, and is rather prone to become purulent, so that special vigilance is here necessary. In the adult, the effusion is often subacute or chronic, and at first sero-fibrinous, but only after a time acquires a sero-purulent or purulent character, so that at this period of life there is less urgency as regards immediate operation. In any case, and at all ages, if the fluid be sero-fibrinous, the close treatment is invaluable; and it may be repeated at short intervals, if necessary several times, provided that on neither the first nor on any subsequent occasion no large accumulation of fluid be knowingly permitted, for fear that the lung should be too much or too long compressed. If the fluid should occupy the lower third of the distended pleural cavity, and shows no disposition to become absorbed, it should be removed; and if effusion again take place, it should not be allowed to reach the same amount as before. On the other hand, in any case and at all ages, if the sero-fibrinous effusion is followed by a sero-purulent or purulent effusion, or if it be either of these at first, then the continuance or the use of the close method is accompanied with risk. In either event time will most probably be lost; pus will most likely collect afresh; other complications may arise in the pleural cavity or wall; the lung if not already diseased may become so—that is, inflamed, tuberculous, excavated, or perforated; or it may not only suffer from prolonged compression, but get covered by false membrane, and so be unable to be refilled with air, when at length the open method of treatment becomes inevitable. Instances of sero-purulent and purulent effusions, especially in acute pleurisy in children and in young persons, treated by one or more prompt aspirations, and therefore under the close method, not unfrequently end favourably; but the unfavourable cases are at least as numerous; and, as a general practice, I should recommend in them the immediate or very early adoption of the open plan of treatment.

The *close* treatment. The skin of the patient and the hands of all concerned in the operation having been disinfected, an aspirating needle thoroughly cleansed and then dipped in carbolised oil should be entered through the sixth interspace on the right side, and the seventh on the left in the mid-axillary region, or wherever previous antiseptic exploration has revealed the existence of fluid. The exact place chosen should be about on a level with the fifth space in front, the spot of "spontaneous perforation"; it is, I believe, needless to puncture lower down, unless the effusion be specially situated and circumscribed there. The exhausting apparatus should work easily and smoothly, and the force employed be moderate; the fluid should not be drawn off too suddenly, nor too much at one operation. From eight to sixteen ounces in a child, and from twelve to twenty-four ounces in an adult, are, as a rule, sufficient. Symptoms of constriction, dyspnœa, or syncope, of course, render it necessary to arrest the flow at less quantities than these. If a large quantity be drawn off, the vessels of the hyperæmic pleura lose support, and fresh effusion may rapidly ensue; if it be drawn too violently or suddenly, rupture of these vessels may occur, followed by hæmothorax, or, in the case of a softened condition of the lung, by rupture of its air vesicles with pulmonary emphysema, or rupture of its coverings and pneumothorax. Early, slow, gentle, and limited aspiration will probably be followed by absorption of the residual fluid, whilst if violently performed it is more likely to lead to renewed effusion or suppuration. Complete evacuation, as advocated by the followers of Trousseau, may bring about a sudden cure, but a survey of many recorded cases proves that the greatest success has followed early, cautious, and partial removal of the fluid, repeated, if necessary, until absorption has set in. In no case of simple sero-fibrinous effusion would I inject anything whatever into the pleural cavity. If the fluid be highly fibrinous or unusually viscid, or if it already contain a few delicate coagula, which may be the case if any red blood-corpuscles have been extravasated into it, a full-sized aspirating needle should be used, provided with a probe for clearing it, if the suction power of the aspirator be insufficient to dislodge the clot. During aspiration, and also when removing the needle and closing the puncture with collodion, no air must be admitted into the chest; indeed, this is of the essence of the operation of aspiration, and, together with the command it gives over the contents of the pleural cavity, constitutes its main advantage as compared with paracentesis with a fine trocar and cannula, however these instruments may be adapted, modified, or guarded. Aspiration thus carefully performed will but seldom, in otherwise healthy persons, give rise to suppuration; but if a trocar and cannula be used, however fine or specially constructed, air will often pass in along or by the side of them into the pleura, and almost certainly so in the repeated use of these instruments. It has been suggested, under the close method of treatment, to irrigate and even wash out coagula by alternately filling and emptying the pleural cavity through a larger aspirator or cannula; and Dr. Hicks has brought here for your inspection a most ingenious and perfect double aspirating bottle for that purpose. But as a single residual clot will suffice to set up continuous irritation, I am not inclined to trust, as a rule, to such a method, although it may be tried; but where the coagula are numerous or large, the open treatment had better be promptly adopted.

The *open* treatment.—Under this plan, which is preferable in sero-purulent and purulent effusions, and necessary in their several grave complications, paracentesis with a large trocar and cannula is a safe and less formidable proceeding than an incision with a scalpel, which is a more efficacious though more serious operation. In either plan, the strictest antiseptic precautions should be taken in operating. The use of the spray and free drainage into antiseptic dressings, together with suitable irrigations and injections, if these be advisable, are to be recommended.

The point of entrance into the pleural sac, if in the mid-axillary region, provided there is fluid there, should not be lower than the sixth space on the right side, and the seventh on the left, which places, as already stated, are about on a level with the common spot of "spontaneous perforation," in the fifth interspace in front. But I advocate the selection, at all events in the adult, of the fifth space, at or as near as possible to that spot, regard being necessarily had to the position of the pericardium on the left side. I would advise in any case in the adult a close examination of the fifth space beneath the nipple; for I should be prepared to find in a certain number of instances of purulent effusion evidences of some *thinning* of the chest walls there, long before there was any distinct swelling or redness, or other sign of spontaneous perforation; and, in such an event, it would certainly be right to operate at that place. Cases of spontaneous perforation or of artificial incision through the point of threatened perforation, provided that the opening of the pleural sac has not been too long delayed, and that free drainage has been afterwards secured, appear by the records, so far as I have been able to discover, to end as favourably as those in which paracentesis or incision has been practised in the seventh, eighth, or ninth interspace, or even more favourably.

Cases of spontaneous perforation in which the aperture has been subsequently opened freely appear also to do well. Even those in which a posterior counter-opening has been regarded as necessary, show as favourable results as those in which counter-openings have been made after a first operation in some lower interspace. On the contrary, an analysis of many recorded cases has shown me, what agrees with my own experience, that many cases in which lower openings have been first made have been unusually obstinate and protracted, some of these, as I know, remaining still uncured. Lastly, it may be added that many authorities (Roser, Billroth, Wagner, Professor Morgan, &c.) expressly indicate the fifth interspace as the proper situation for paracentesis, incision, and drainage, without referring this practice to an imitation of nature. Whilst, therefore, laying down a rule never to perform open paracentesis or incision lower than the sixth or seventh interspace at the sides of the chest, I would, in the adult, venture to recommend in preference the fifth space nearer to the front.

The idea that we necessarily drain the pleural cavity most effectually by making a puncture, or an incision at the *lowest* available point *behind*, is I believe, a chimera. Even in an ordinary abscess the surgeon does not always make an opening at the most dependent part, but preferably at the thinnest spot, where the abscess is pointing, or seems likely to point, and towards which the sides of the abscess afterwards most naturally collapse. In most cases, this spot is opposite the middle of the abscess, and not at its dependent part, and, as already observed, if the pleural sac be over-distended, the lung being compressed towards its root, the mediastinum, heart, and pericardium pushed sideways, and the diaphragm downwards,—then the fifth interspace below the nipple is approximately opposite the middle of the great pleural abscess, towards which central point the ribs endeavour to recoil, and the heart, lungs, and diaphragm all tend to move back. In the condition of health, the topmost arch of the diaphragm on the left side may reach as high as the level of the fifth space, and on the right side that of the fourth; and when the distended pleura is being emptied, the chest walls fall in and the depressed diaphragm ascends, whilst the costal and diaphragmatic surfaces of the pleura, inflamed and covered with organised lymph or granulations, approximate and, if not prevented, will cohere. If, in any case, a puncture or an incision has been made in the eighth or ninth interspace, or even in the seventh, the aperture has a marked and troublesome disposition to be closed by the diaphragm; and if a drainage-tube has been inserted, and a long one then becomes necessary, the lower part of the tube, placed between the chest wall and the movable diaphragm, keeps up local irritation, maintains suppuration, and prevents the curative process of adhesion. In four cases of chronic empyema, recently or now in this hospital, in which the chest was opened in the seventh, eighth, or ninth spaces, and in every other case of low operation which has come under my observation, the aperture has tended obstinately and prematurely to close, whilst the drainage-tube has been nipped between the diaphragm and the ribs, and has interfered with the wished-for cohesion between the costal and diaphragmatic pleura. In all such cases, I find that a long probe passed into the aperture is immediately deflected upwards by the diaphragm along a narrow space, and then enters a wide cavity higher up. It is, in fact, the lower part of the distended pleural sac which first collapses, whilst the upper part contracts less quickly, and remains open, continuing to yield pus. This constitutes a different condition from that of an abscess or sinus in the soft parts, with walls equally collapsible at all points. A long drainage-tube in the chest, and such a tube is sometimes twenty inches in length, is undoubtedly a great evil. If the puncture be low down, a long one must be employed to drain the upper cavity; but by making the opening as high as is practicable, say, at or level with the common spot of spontaneous perforation, a very short tube will be sufficient to drain the upper part of the cavity, whilst the lower part will close up by the contact and gradual adhesion of the diaphragm with the chest wall, and requires no drainage. I am satisfied that the evidence is irresistible as to the results being better, with the higher opening and the shorter tube, than with the lower opening and the longer tube. It, moreover, seems to me that counter-openings to drain the back part of the pleura, unless the empyema be localised there and circumscribed, are not so frequently necessary with a high as with a low opening; for in the latter case the drainage-tube interrupts the healing process at the back of the chest. Even if a posterior counter-opening should be required, it should be made higher up than is usual—say, in the seventh or sixth interspace instead of the eighth or ninth.

The place of operation being determined on either by paracentesis with a large trocar and cannula or by incision into the pleura, I believe it is better to make the previous cut through the skin across the two adjoining ribs and the intercostal space, not parallel with them; for, on the former plan, the edges of the skin are less likely to be drawn in between the ribs as the chest wall collapses, and so to cause future trouble and pain. The fifth interspace being widest just to the inner side of the mammillary line, and much wider than any of the lower spaces, the puncture or incision, made of course for the avoidance of the intercostal artery near the upper border of the lower rib, is more easily accomplished than lower down. Moreover, as the fifth and sixth ribs are more fixed than those below them, there is less chance of the opening being subsequently narrowed by approximation of the bones, which in lower situations often proves a great embarrassment. These considerations constitute additional recommendations for using the fifth interspace.

In an acute general empyema, such as is most common in children and young persons, where the effusion consists of laudable though whitish pus, the operation, whether by puncture or incision, performed preferably on the antiseptic method, should be followed by the introduction of a wide but short silver or plated drainage-tube, which should just enter the pleura and may be made to curve slightly upwards. No injection into the pleura is necessary, and the use of one may be most injurious; for complete evacuation is not immediately needed, and violence would irritate the tender and highly vascular lining membrane, might cause hemorrhage, or determine destructive inflammation and sloughing, as is shown in this wax cast taken from a fatal case in which the pleura had been opened. If an acute empyema be treated early and gently, with a free opening and good drainage, the patient being kept recumbent on the face or side, the pleural cavity will gradually empty itself effectually, its diaphragmatic, mediastinal, and costal boundaries will assume their normal positions and the lung be again inflated. Some adhesions will probably be formed, but the empyema will be cured; and in children and in young persons the lung appears in time even to expand to its normal size. The important condition, however, is this: that the operation has been performed early—that is, as soon as ever suppuration has been detected, before the lung has been much or long compressed or otherwise injured. Hence it is imperative at once to explore, wherever a suspicion of suppuration exists.

But, as shown in the numerous pathological preparations and drawings before us, various complications are met with in empyema. If, for example, the fluid effused, instead of being a uniform laudable pus, should contain coagula or large flakes, in which latter case the pleura is probably also covered with false membrane, these must be washed out, by the aid of a syringe and full-sized catheter, with a solution of permanganate of potash or boracic acid; otherwise, by their presence or by their subsequent decomposition, they will impede the cure, or convert the case into one of obstinate chronic empyema. If the pus be enclosed in distinct loculi, whether few or many, the washing out may serve to break down the septa, and then dislodge and expel them; or a long silver probe, straight or bent in various curves, may be first gently manipulated, so as to rupture and detach the membranes; or a free opening being of great moment, the trocar puncture may be dilated or enlarged by incision, the finger introduced, the cavity explored, and any membranous flakes and torn laminæ be displaced and removed by washing. If, as in a child, the intercostal space operated upon is too narrow; or if, as in older patients, the adjoining ribs have fallen in and become approximated during the progress of the case—a complication difficult to prevent, and a source of great discomfort and pain in the introduction and wearing of a permanent drainage-tube—a portion of a rib may, as shown by Dr. Thomas, Kœnig, and others, be resected, with great advantage; or, as I have done in the adult, the upper two-thirds of the breadth of a rib may be trephined, in order to give more room for exploration, evacuation, ablution, and prolonged drainage. Similar steps might also be had recourse to, to anticipate and provide against that approximation of the ribs which is inevitable as the collapse of the chest wall goes on, and of which I show you a specimen here.—*Lancet, March* 4, 1882, *p.* 337.

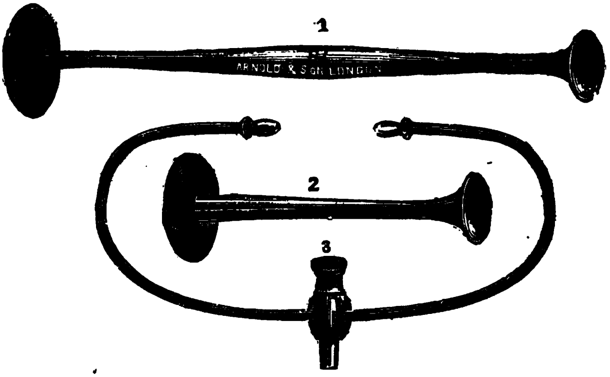
29.—ON A NEW CONVERTIBLE STETHOSCOPE.

By JOHN WARD COUSINS, M.D.Lond., F.R.C.S., Surgeon to the Royal Portsmouth Hospital, &c.

The new stethoscope, which I beg to introduce to the notice of the profession, is a very simple contrivance, possessing the advantage of the double and single instruments in combination. The single stethoscope is now made in many forms, and with a variety of materials; still there exists a general preference in favour of the old-fashioned wooden instrument. At the present time, it is almost universally used by practitioners; and, no doubt, it serves, in a vast majority of cases, every purpose for correct diagnosis. But the practical question arises: Why is the single instrument so generally employed, instead of the double stethoscope? The preference, however, appears to me capable of very ready explanation. The double stethoscope, in the form in which it has hitherto been presented to the profession, since its introduction by the late Dr. Leared, is a somewhat costly instrument, and certainly less portable than the ordinary wooden tube. Few medical men are inclined to carry two forms of the same instrument, especially when one has been used habitually and successfully for many years. Moreover, the introduction of the ear plugs of the binaural stethoscope into the auditory canal sometimes causes uneasiness and discomfort, and this is frequently observed when they are supported in position by an elastic band. The external meatus is of very variable capacity in different individuals, and this important fact is often overlooked. It is essential that the plugs be perfectly adapted to the size of the canal; for, when they are too large, they cannot be retained; and, when too small, they occasion unpleasant sensations and friction-sounds in the ears. These inconveniences, however, are only temporary, and can be readily overcome by frequent practice with a well selected instrument.

The double stethoscope presents many special advantages in the physical examination of the chest. Two ears are certainly better than one for discriminating sounds, just as two eyes are better than one for purposes of vision. It is often important, for the sake of accurate diagnosis, to hear sounds emanating from a region of the chest with two ears simultaneously; and the value of the binaural stethoscope is very evident in distinguishing obscure and feeble sounds, and also in defining the precise seat of their greatest intensity. The double stethoscope is certainly the best instrument for the student. It is a good plan to commence the study of auscultation with both ears, for this renders a practical acquaintance with the details more easy. The auditory power is very variable among different individuals, especially an acuteness of hearing for recognising low and feeble sounds; and experience proves that this power of ready discrimination is far more effectively cultivated with two ears than with one. By the same means, too, the faculty can always be improved and sharpened; and, with those who have long confined themselves to the use of the single instrument, and who have hitherto had no confidence in the binaural, it will be a great help to conduct an examination by the aid of both instruments alternately. A comparison between the auditory impressions can be thus readily obtained, the old prejudice will soon be dissipated, and the great advantage of using both ears fully recognised. I have often heard my friends remark, that "they do not like the double stethoscope;" but this dislike has been generally associated with no real effort to learn its great practical value. It is admitted by all accomplished auscultators to possess many advantages; and I am bold enough to express the opinion that it will, before long, take the place now occupied by the old-fashioned tube.

The "convertible stethoscope" is a very simple arrangement for promoting the practical study of auscultation with both the single and the double instrument. It consists of a flat ear-piece; a shaft, nine inches in length, separable into two parts; and a flexible tube, with perforated wooden ear-plugs. These separate parts are all adapted to each other by the same simple joint; so that, by fitting them together in different combinations, four stethoscopes are produced.



1. An ordinary short stethoscope (Fig. 2), which is especially serviceable for the dorsal examination of patients confined to bed.

2. A long single stethoscope, adapted for subclavicular auscultation (Fig. 1). This form of instrument is often very useful in practice; for, by its length, the head is kept away from the face of the patient, and it also prevents uncomfortable stooping over the bed.

3. A double stethoscope (Fig. 3). The shaft of the short single tube completes the instrument. The ear-plugs are retained in position, without pressure, by adapting them to the size of the external auditory canal, and not by fixing them in the ears, as in the ordinary binaural, by means of an elastic band attached to the ear-tubes. This method removes at once some of the most common objections raised against the instrument. It prevents the uneasy sensation of pressure in the meatus, and also the friction-sounds which are often so perplexing to the student. The ear-plugs, moreover, are made in several sizes, to suit the varying capacity of the canal; and this is a little practical matter very necessary to remember in selecting an instrument. This will be found a very handy form of stethoscope. The chest-sounds can be examined with both

ears, and then again with either ear alternately, by simply compressing the elastic tubes between the finger and thumb; in this way, the single and double auditory impressions may be conveniently compared and studied.

4. A double stethoscope, with an auxiliary cranial conductor. The conductor is formed by placing the end of the long shaft between the teeth, or on the forehead. In the latter method, the ear-piece is fixed on the shaft, for the purpose of increasing the area of the conducting surface.

The value of this additional channel for the purposes of auscultation can be very readily estimated by experiment. I have employed it for many months; and, it is my opinion, that the sonorous continuity of the cranial bones and auditory nerves may be utilised, as an auxiliary channel, during many kinds of physical examination. It gives a remarkable clearness and distinctness to the auditory impressions; it assists in the definition of many feeble sounds; and it is particularly useful in the diagnosis of cardiac disease. The utilisation of this channel to increase the auditory impression is theoretically correct. It is a well-known fact that vibrations, thus conveyed, not only produce an effect independently of the tympanic apparatus, but also that they appear to become intensified by an obstruction to the transmission of sound-waves through the middle ear, caused either by aural disease or by intentional plugging of the meatus. Now, when the double stethoscope is used in conjunction with the auxiliary conductor, the ears are in the most favourable condition for the reception of sonorous impressions in this direction, as the canals are artificially plugged by the instrument itself. The activity of the tympanic apparatus, however, is not interrupted by this condition; for, when vibrations are transmitted through both routes at the moment, they react favourably upon each other; and the artificial obstruction of the canals, while increasing the direct cranial conduction, also intensifies the whole auditory impression. There may be considerable individual differences as to the susceptibility of the labyrinth for feeble vibrations, excited directly through the skull, and these variations may be detected without any apparent aural defect; still, instances in which persons of normal hearing power find a difficulty in appreciating auxiliary conduction will be extremely rare, and will always be removable by a little trouble and practice. The method of utilising the sonorous continuity of the skull and the auditory nerves, will prove a valuable aid to any who labour under unilateral tympanic disease. Sound, transmitted through the conducting apparatus of the middle ear, produces a greater effect upon the organ than when conveyed through any other medium; still, when the tympanic apparatus is injured, the vibrations of a solid body, in contact with the cranium, are heard distinctly louder on the injured side. This effect, which is constantly observed in chronic aural affections uncomplicated with disease of the nervous structures, is caused by the repeated reflections of the waves of sound in their outward passage from the tympanum towards the meatus. The auxiliary conductor will thus form a very serviceable addition to the double stethoscope, for those auscultators who are unfortunately injured in the conducting apparatus of one ear. Under this condition, the ordinary channel of sound is obstructed, still the vibrations are not lost, but are utilised by direct transmission to the auditory nerve; and, in this way, they materially intensify the sensation received by the sound organ.

5. The convertible stethoscope is capable of another modification. It can be used as a differential stethoscope, by substituting two elastic chest-tubes for the single chest-piece. The double tube, introduced by Dr. Scott Alison some years since, is now used by many experienced auscultators in the diagnosis of cardiac disease. By its aid, sounds proceeding from two distant parts can be conveniently distinguished and examined, and a comparison obtained between any two points on the surface of the chest. Sounds are thus distinguished and separated by the ear, their position defined, and their relation to each other determined, both as regards character and time. It is a good plan to examine both points of the chest simultaneously, and then alternately; and also to obtain single and double impressions of the different sounds. For all the purposes of accurate definition and refined auscultation, the differential stethoscope is a very valuable instrument; but its application requires careful study.

In conclusion, I beg to express the hope that the convertible stethoscope may prove of value to some of my professional brethren. It is a very portable instrument, of few separate pieces, although it admits of many combinations; and I trust it will help the hardly-pressed student of the present day to educate his ears, and obtain a practical knowledge of one of the most useful branches of our science. The instrument is made by Messrs. Arnold and Sons, of West Smithfield, and is supplied in a small case, so that it can be carried in the pocket very conveniently, and with more safety than the ordinary stethoscope.—*British Medical Journal, Jan.* 14, 1882, *p.* 45.

30.—CASES OF CONTAGIOUS PNEUMONIA.

By W. A. PATCHETT, M.R.C.S., Shaw, near Oldham.

On reading Dr. Daly's article on Contagious Pneumonia in a recent number of the Lancet (*vide Retrospect*, vol. 84, p. 333), I felt that I ought to publish some most extraordinary cases which came under my care a few years ago, where a whole family of five persons died one after the other, in less than a fortnight of typical and uncomplicated pneumonia, and can, I think, hardly be explained, except on the supposition that there is such a disease, however rare, as contagious pneumonia.

Four brothers (James S——, John S——, Joseph S——, Wm. S——) and a sister, all unmarried, lived together; they kept a farm, and had lived together at the house all their life. The house was well and healthily situated on a steep hillside, and all the sanitary surroundings, ventilation and water-supply, were everything that could be desired, so that there could be no suspicion of any septic epidemic pneumonia or of pneumonia from blood-poisoning. The previous health of each of them had been exceptionally good—in fact, they did not remember having had any illness previously. A detailed account of each case would occupy too much space; it will be sufficient to give an epitome of the cases.

On Jan. 13th, 1876, I was called to James S——, aged seventy-three, the eldest of the family. I found him suffering from typical pneumonia, with crepitation over the front and consolidation of the base of the right lung, a temperature of 104·4° F., a flushed and dusky face, a pungently hot skin, short hacking cough, with rusty-coloured sputa. He had been ailing for three days, and began with shivering and pain in the side. The constitutional symptoms did not appear very urgent, but he rapidly got worse, and died on the 16th, or six days from the commencement of his illness.

On the morning of the death of James S——, John S——, aged sixty-six, who the day before had complained of rigors, complained of pain in the right side, and cough with rusty sputa. He was found to be suffering from right pneumonia, which the day following had extended to the left lung. He also did not appear to suffer much, but he said he should not get better. The disease assumed an asthenic form, and he quickly sank and died on the 19th, or in three days.

On the morning of the 20th, Joseph S——, aged sixty-three, complained of pain in the left chest, cough, with rusty sputa; and on examination there were crepitation over the front of both lungs, and dulness over both bases; temperature 104°. The day before he had felt quite well, and was following his usual work about the farm.

On the evening of the 20th, W. S——, aged sixty-four, on going to bed complained of shivering and feeling cold, and the next morning, on my visit, he was found to be suffering from double pneumonia. In neither of these last two cases did the symptoms appear very urgent, but they were both very despondent, and had a presentiment that they would die the next day, and they did die on the evening of the 22nd.

Miss S——, the sister, aged sixty-one, had been in attendance on all her brothers during their illness, and she seemed to keep up her strength and health remarkably well; but on the 23rd she was seized with right pneumonia, and she quickly sank, and died on Jan. 26th.

As may be imagined, these cases created a profound sensation in the neighbourhood, the popular opinion being that as the brothers and sister had lived so affectionately together all their lives, the death of the eldest had so affected the others that they died "broken-hearted." Dr. Howard (who saw the cases daily with me) and myself were quite unable to explain such an occurrence. When the second brother sickened and died of pneumonia we thought it was only a coincidence, and that probably nervous shock caused by the death of his brother had contributed to his death; but when the remainder of the family sickened and died of a disease hitherto considered non-contagious, it seemed to point to something more than coincidence merely.—*Lancet, Feb.* 25, 1882, *p.* 305.

31.—ON THE TREATMENT OF CHRONIC DYSENTERY BY VOLUMINOUS ENEMATA OF NITRATE OF SILVER.

By Stephen Mackenzie, M.D., F.R.C.P., Physician to and Lecturer on Medicine at the London Hospital.

In the treatment of ulceration of mucous membranes within reach of sight and touch, all practical physicians and surgeons are convinced of the great importance of local applications, whether the seat of the disease be the pharynx, the larynx, the eye, the cervix uteri, or elsewhere. In these situations, whether the ulceration be of constitutional or local origin, we employ local treatment, with or without internal medication, and as the effects of treatment can be observed, no one doubts its efficacy. All practical surgeons are assured of the beneficial influence of applications of nitrate of silver, and other mineral astringents and mild escharotics in treating inflammations of mucous membranes. How different is the treatment of dysentery from that of inflammation of the upper part of the alimentary tract. It may, of course, be remarked that treatment which is obviously beneficial to such parts as our hands can reach, cannot, on account of physical difficulties, be applied to the length of the colon. But this difficulty is not wholly real. With a view of rendering our practice in the treatment of dysentery more successful, and more in accordance with our procedures elsewhere, Dr. Horatio Wood, of Philadelphia, has suggested the use of a large enemata of nitrate of silver, so as to bathe with a solution of this salt the whole mucous membrane of the colon. This treatment he has appropriately designated "the rational treatment of dysentery." I am aware that enemata have long been employed in the treatment of dysentery, but the importance of *large* enemata has not been insisted on, and their use is not general. Ringer, Gairdner, Bristowe, Niemeyer, and others recommend the employment of enemata, and in some instances of nitrate of silver, but none enjoin the use of such large enemata of nitrate of silver as recommended by Dr. Horatio Wood.

"The disuse of local applications in dysentery is largely, no doubt," Dr. Wood observes, "the result of our former inability to make use of applications to any other than the extreme lower portions of the colon. By the use of forced enemata, so-called, we are now, however, able to reach every part of the large intestine. In giving such injections it should be first remembered that the name is a misnomer; that no force should ever be used. The patient should be brought to the edge of a hard bed, placed in a position somewhat resembling that for lithotomy, his buttocks resting upon a hard pillow in such a way as to elevate the pelvis, and cause the injected fluid naturally to flow downwards and inwards. A well-oiled, smooth, somewhat flexible, hard tube, with openings at the side (an œsophageal tube will answer well) and a closed end, must then gently and slowly be introduced from eight to twelve inches into the rectum. The free outer end of this may be connected with a Davidson's syringe, and the fluid thus be slowly pumped in. A better plan is to unite it with a flexible india-rubber tube, in the end of which a funnel is inserted. This being elevated five or six feet, the water is poured in, and by its own weight, with irresistible gentleness, forces its way into the gut. Instead of a funnel being used, the tube may be so arranged as to empty a bucket or other reservoir of water, placed five or six feet above the patient. A direct connexion may be made, or the principle of the syphon taken advantage of. Finally, the so-called fountain syringe may be substituted. In any case the liquid should be about the temperature of the body, so as not to provoke peristalsis by the stimulus of heat or cold."

Dr. Wood writes that whilst some considerations would lead us to expect variety in the character and strength of the applications would be likely to be serviceable, his experience of throat affections led him to select nitrate of silver in the first instance, and he has been so satisfied with its results that he has employed no other. Drachm doses have, in his hands, never occasioned constitutional symptoms, and less than forty-grain doses have not accomplished much good. In one of my own cases, to be narrated, a single enema of thirty grains of nitrate of silver, in three pints of water, caused the complete cessation of chronic dysentery that had lasted two years. This I regard as exceptional. I believe that, as a rule, at least a drachm of nitrate of silver to three pints of water should be used, and I have employed as much as a drachm and a half of nitrate of silver to this quantity of water with good result and without danger. Dr. Wood properly discusses the possible effects of the application for a longer period than occurs elsewhere, of so large a dose of nitrate of silver to an absorbent surface, but has never seen the least inconvenience arising from it. My experience is in entire accord with his. He suggests that, in case of the enema being retained, and fear arising as to the toxic effects of a large dose, a solution of common salt should be at hand to inject, and neutralise the nitrate of silver. In two cases, in my absence, my house-physician thought it desirable to do so.

I myself have been led to try perchloride of iron instead of nitrate of silver, as the former would be wholly destitute of the dangers entailed by the use of the latter, and any absorption which took place would be an advantage rather than the reverse. But my results have not been nearly so good with iron as with silver. I have not experienced any practical difficulty, inconvenience, or the least danger with these large and comparatively strong injections of nitrate of silver, whilst the results have been so encouraging that I am anxious the treatment should be tried on a larger scale, and by other observers. It will, of course, be understood that this plan of treatment is not suitable for diseases of the intestines above the ileo-cæcal valve.—*Lancet, April* 22, 1882, *p.* 641.

DISEASES OF THE URINARY ORGANS.

32.—ON THE VARIOUS FORMS OF RENAL TUBE-CASTS AND THEIR DIAGNOSTIC SIGNIFICANCE.

By George Johnson, M.D., F.R.S., Prof. of Clinical Medicine; Senior Physician to King's College Hospital.

The microscopic examination of the urine secreted during the different stages of the various forms of renal degeneration, which are usually included under the generic term "Bright's disease," affords evidence which, rightly interpreted, is of great value as to the morbid changes which are in progress within the kidney. This evidence is of a peculiarly interesting character, and is based upon the following facts: 1. That the glandular structure of the kidney is tubular; 2. That, while some structural changes are interstitial and intertubular, the most constant and important of the morbid processes are intratubular; 3. That portions of the various morbid products, after being moulded within the tubes, become loosened, and then, being detached and carried on by the secreted current of liquid, appear in the urine in the form of cylindrical casts of the tubes. 4. These tube-casts vary much in size and form, and in the materials of which they are composed; and they throw much light upon the condition of the tubes from which they have escaped.

Before proceeding to describe and interpret the various forms of tube-casts, let me give one or two hints as to the best mode of finding and preserving them. If the casts be very numerous, they may be found at once by placing a drop of the urine under a power of 200 or 250 diameters; but, as a rule, it is desirable to put about four ounces of the urine into a conical glass, and allow it to stand for from twelve to twenty-four hours. At the end of that time, a sediment has usually formed; it may be only a slight or scanty cloud, or a more or less copious and dense deposit. If no sediment be visible, few or no tube-casts will be found even after the most careful and prolonged examination. A few drops of the sediment, taken up with a pipette, are then placed in a shallow cell made by cementing a thin ring of glass or metal on a glass slide; or a cell may be made of a ring of cement, such as Hollis's glue or Brunswick black. The cellful of liquid being covered by thin glass, and the excess of liquid removed by a cloth or blotting-paper, is now ready for examination. The examination is much facilitated by the use of the shallow cell. When the sediment is scanty, it is a waste of time to be searching for tube-casts in a fraction of a drop of the urine which alone can be retained between two flat pieces of glass.

And here let me say that the various forms of tube-casts may, with a little care, be preserved for an indefinite time. Some of the specimens which I shall presently show you were mounted more than a quarter of a century ago. I shall direct your attention especially to some specimens from which were made drawings to illustrate a paper of mine on the Forms and Stages of Bright's Disease, which was published in the forty-second volume of the Medico-Chirurgical Transactions. At page 161 of that volume you will see microscopic drawings of the various forms of tube-casts which were found at different stages of a prolonged case of Bright's disease. The specimens were mounted at different periods of the year 1856; and, if you compare them with the woodcuts, you will see that to this day they retain their characteristic appearance. These specimens of tube-casts, like most of those that I possess, have been preserved in pyroxylic spirit—one part of spirit to nine of water. The mixture of spirit and water becomes milky; but, after standing for some days, with an occasional shaking, it may be cleared by filtering through paper. I have lately been using a saturated solution of salicylic acid for the same purpose; and, so far as I can judge from a comparatively brief experience, it appears to answer well. It preserves the casts with little or no change of appearance.

In order to understand the significance of tube-casts, you have to bear in mind the structure of the kidney, with which I must assume that you are quite familiar. To suppose the contrary would be an insult to men studying in the school where, forty years ago, Bowman first demonstrated the structure of the Malpighian bodies. The basis of all tube-casts is an albumino-fibrous material derived from the blood, which, when it exists alone, forms a transparent structureless so-called "hyaline cast." "Hyaline casts" may be "small" or "large"; the latter being about twice the diameter of the former, and having a more sharply defined outline. What is the explanation of this difference of diameter? Not, as is sometimes carelessly asserted, the difference in the size of the tubes in which they have been moulded. The vast majority of the casts, both large and small, have been formed within the convoluted secreting tubes of the cortex, which are remarkable for their uniform size. The different diameter of the casts is explained by the fact that the small casts have been moulded within the lumen—the clear central canal—of convoluted tubes which retain their lining of glandular epithelium; while, on the other hand, the "large hyaline cast" has been formed within the basement-membrane of a tube the epithelial lining of which has been destroyed and displaced by the hyaline material of the cast. The large casts, therefore, point to a greater structural change within the tubes than is indicated by the small casts.

In some of the specimens which I have placed under the microscope, you may see a granular cylinder, composed of more or less altered epithelium, enclosed in a casing of the hyaline material, which, apparently having passed through the basement-membrane, has displaced and surrounded the epithelial lining of the tube.

Another interesting fact is, that many of the hyaline casts retain more or less of the convoluted form, which proves that, although in their exit from the kidney they have passed through the straight collecting tubes of the medullary cones, they were moulded in the tortuous secreting tubes of the cortex.

No doubt many of the long convoluted casts which, in sections of the cortex, may be seen filling the uriniferous tubes, become broken into short fragments during their passage through the straight tubes of the cones; and an indication of this process is often afforded by a fissure extending more or less deeply from the concave side of a curved hyaline cast. The explanation of this is, that a convoluted cast, in passing through a straight collecting tube, has been partially torn asunder during the straightening process to which it has been subjected. I have hitherto spoken only of large and small hyaline casts. When, as frequently happens, in cases of acute congestion of the kidney, blood escapes from ruptured Malpighian capillaries into the uriniferous tubes, some of the blood coagulates within the lumen of the tubes, and afterwards appears in the urine in the form of "blood-casts." These casts have the same diameter as the "small hyaline casts."

But the most constant and characteristic structural change resulting from the acute renal disease, which is excited by scarlet fever, by exposure to cold and wet, or other well known exciting causes, consists in a rapid new formation and desquamation of the glandular epithelium of the convoluted tubes. This is the essential feature of the disease, which I originally designated "acute desquamative nephritis." The desquamated gland-cells become entangled in the tube-casts, which we know as "epithelial casts."

All the forms of tube-cast which I have mentioned—namely the "small and large hyaline casts," "blood-casts," and "epithelial casts" may be seen in varying proportions in every typical case of acute desquamative nephritis.

During the progress of acute nephritis, it not unfrequently happens that some of the exudation-products undergo an oily transformation, and oil accumulates in the gland-cells. This change is indicated by the appearance of "oily casts" and "oily cells" in the urine. A few oily casts and cells mingled with the other casts which I have described have no specially unfavourable significance; but when, after albuminuria has continued for several weeks or months, the oily casts and cells preponderate, or even entirely replace the other forms, there will be reason to fear that the kidney is undergoing an incurable fatty degeneration; and, if death occur at this stage, the kidney will be found enlarged and pale, while, through its cortical portion, yellow granules of fatty degeneration will be found scattered. This constitutes the large, white, granular, fat kidney.

When acute desquamative nephritis has passed into a chronic state, there may, for a number of years, be little or no disorder of the general health; but the urine is persistently more or less albuminous. Tube-casts may be entirely absent, or there may be a light cloudy deposit containing small hyaline casts with an occasional oily cast, or oil-containing cell. As the disease makes progress, the urine gradually loses its normal sherry tint, and at length becomes more or less colourless. If, at the same time, the deposit become more dense and copious, and contain large granular and large hyaline casts, indicating extensive destruction of the uriniferous gland-cells, we have, in these appearances, evidence that a process of granular contraction has begun in the cortical portion of the kidney; and the gravity of the prognosis is greater in proportion to the amount of this microscopic sediment in the urine.

Glomerulo-Nephritis.—The acute renal disease which results from scarlet fever has not always the character of desquamative tubular nephritis. There is a form of nephritis to which attention was first directed by Dr. Klebs, under the name of "glomerulo-nephritis." In this form of disease the chief anatomical change consists in an accumulation of leucocytes within the Malpighian capsules. Long before the publication of Klebs's observations, I had noticed the occasional appearance in the urine of small casts entangling leucocytes. I have here a specimen of such casts, mounted in 1856. The case was fatal, and I have preserved a section of the kidney; in which may be seen the Malpighian capsules rendered opaque by the accumulated leucocytes. I confess, however, that, until I became acquainted with the observations of Klebs, I did not appreciate the full significance of these appearances.

In my *Lectures on Bright's Disease*, published in 1873, I have suggested what I have no doubt is the true explanation of the sources of the "white cell casts," or, as I then called them, "exudation-cell casts"—namely, that the leucocytes or white blood-cells, having migrated through the walls of the Malpighian capillaries, subsequently became moulded into small cylindrical casts within the central canals of the convoluted tubes. The white cell-casts are the diagnostic indications of the existence of glomerulo-nephritis, and they are the complement and counterpart of the anatomical changes within the Malpighian capsules.

Although the two forms of disease here referred to—namely, tubular desquamative nephritis and glomerulo-nephritis—may exist apart, yet I am convinced, from clinical observation, and in particular from microscopical examination of the urine, that they are often combined in various degrees in the same subject. The epithelial and other forms of tube-cast which I have described as diagnostic of desquamative tubular nephritis being frequently associated, in varying proportions, with the white cell casts of glomerulo-nephritis.

The Small Red Granular Kidney.—The urine secreted by the small red granular, or, as it is sometimes called, the gouty kidney, is usually copious, of low specific gravity, and more or less albuminous, though the albumen is not rarely scanty, and at some periods of the disease even entirely absent. The absence of albumen, however, in undoubted cases of granular kidney, is rare, and there is no evidence to show that any case of the disease has passed through all its stages to a fatal termination without the more or less frequent and copious appearance of albumen in the urine. The microscopic evidence of the existence of the small red kidney consists in the appearance of "granular casts," containing epithelial cells from the uriniferous tubes in various stages of disintegration. All so-called granular casts, however, are not composed of disintegrated epithelium. Blood may become disintegrated within the tubes, and appear in the urine as granular blood-casts, distinguished from granular epithelial casts by their reddish-brown colour, and often by their containing some entire blood-corpuscles. So, again, a disintegrated hyaline cast may assume a granular appearance; but, by a comparison with other casts associated with them, and noting the various grades of change, we trace them to their true source.

The presence of granular epithelial casts and scattered epithelial *débris* is evidence that a process of epithelial degeneration and disintegration is going on in the kidney. In the earlier stages of the disease, the granular casts may be occasionally absent, but as the disease makes progress they may always be found, if carefully looked for; and in the later stages, the granular casts are often found associated with large hyaline casts in variable proportions. Not only is the contracting form of renal disease indicated by the microscopic appearances of the sediment, but the number of granular and large hyaline casts, and the amount of epithelial *débris* indicate the rate at which the disease is progressing. The more copious this sediment, the more rapid is the destruction of the gland-cells and the consequent atrophy and disorganisation of the kidney.

Lardaceous Degeneration of the Kidney.—There are no forms of tube-cast which are specially significant of lardaceous degeneration of the kidney. The diagnosis of this disease has to be based upon a consideration of the general history of the case rather than upon the microscopic appearances in the urine. The urine may be copious and clear, and deposit no sediment; and in the earlier stages of the disease tube-casts are never numerous. In most cases, however, a light cloud is deposited, and in this may be found some small hyaline casts, some casts finely granular, and occasionally some oily casts and cells. In the advanced stage of the disease, when atrophy and contraction of the kidney are in progress, the urinary sediment may be copious and dense, containing numerous dark granular and large hyaline casts, similar to those which I have described as occurring when a large white or a granular fat kidney has reached an advanced stage, and is undergoing atrophy and granular contraction.

You may probably have heard or read that more than one recent writer on renal pathology has asserted that no tube-casts can escape from the convoluted tubes of the cortex of the kidney. This assertion is based upon the assumption that all the convoluted tubes pass into the so-called Henle's loops, which are supposed to be too narrow to permit the passage of tube-casts. This anatomical assumption I believe to be erroneous. It is at any rate quite certain that the great majority of tube-casts which appear in the urine have been moulded within the convoluted tubes of the cortex. The casts which evidently have this origin are: 1. The oily casts, the fatty infiltration being as a rule confined to the glandular epithelium of the convoluted tubes; 2. The small white-cell casts, containing the leucocytes, which can have no other source than that of migration through the walls of the Malpighian capillaries; 3. Blood-casts, the result of hemorrhage from the Malpighian capillaries into the commencement of the convoluted tubes; 4. Those large hyaline casts which after their escape with the urine retain the convoluted form, clearly indicating their source in the tortuous tubes of the cortex. 5. The epithelial casts, which contain numerous cells, having all the characters of the glandular epithelium of the convoluted tubes. These unquestionable facts are clearly inconsistent with the theoretical assumption that no casts from the cortical portion of the kidney can escape and appear in the urine.—*British Medical Journal, March* 4, 1882, *p*. 295.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, AND DISEASES OF THE BONES, JOINTS, ETC.

33.—ON THE TREATMENT OF WOUNDS.

By Joseph Lister, F.R.S., Professor of Clinical Surgery in King's College.

To those who know from personal experience what antiseptic measures can do for surgery, it may well seem strange to find some who have taken part in this discussion still assigning them a secondary place in the treatment of wounds. The explanation of this fact is, I believe, to be found chiefly in the success —nay, the brilliant success—sometimes obtained without the use of any antiseptic means whatever. This has been conspicuously the case with abdominal surgery, and especially with ovariotomy. Ovariotomy, indeed, has sometimes been spoken of as a touchstone of the efficacy of the antiseptic treatment. The success which has attended antiseptic ovariotomy has been regarded as a signal proof of the truth of the antiseptic principle. Such, however, has never been my own view. Mr. Spencer Wells and Dr. Thos. Keith achieved results which astonished the world before strict antiseptic treatment was thought of: and when, several years ago, Dr. Keith expressed to me an intention of performing ovariotomy antiseptically, I strongly dissuaded him from his purpose. I knew his already brilliant success; I felt that our spray apparatus was as yet inadequate for the production of a cloud sufficiently large to cover the whole field of operation, and sufficiently fine to avoid needless irritation; and I was also aware that such operations are often both very protracted and very anxious, while in proportion to the duration and the anxiety of an operation is the chance of the neglect of some apparently trivial yet important element in the procedure. And if the antiseptic treatment were attempted in ovariotomy and failed in its immediate object, I felt that it would be not only nugatory but injurious. It seemed to me that in any case of ovariotomy performed without antiseptic measures, there was a contest between effusion from the wounded surfaces and absorption of the effused serum by the uninjured peritoneum. If absorption kept pace with effusion, there was no time for putrefactive fermentation to take place in the effused liquid; but if absorption lagged behind and effusion predominated, the serum accumulated in the abdominal cavity, and, putrefying, gave rise to septicæmia. Now, supposing an antiseptic like carbolic acid to be employed in the operation, the peritoneal surfaces, as well as those of the wound, would be more or less irritated by it, and in proportion to this irritation would effusion be increased and absorbing power enfeebled, till such time as the temporary effect of the carbolic acid had subsided; and if, in spite of the antiseptic means, active septic matter had been introduced, putrefaction and septicæmia would be the natural result.

At the same time I believed that the day would come when strict antiseptic treatment would prove valuable in ovariotomy. Especially did I anticipate that it would permit early operation for small tumours instead of the patient being kept waiting in anxiety, as used to be the case, till the tumour should have attained large dimensions. And in point of fact I think, judging from the records that have come to us from various quarters, and especially from Germany, that strict antiseptic treatment has rendered very great service to ovariotomy; that it has made the surgeon more independent of healthy surroundings, and also has made less essential the excessively minute care during the operation, and the close attention after it which we have all so much admired in the practice of Dr. Keith. Thus surgeons generally have been brought, by the use of antiseptic means, much more nearly towards the high level of Wells and Keith. Even Keith, on at length adopting strict antiseptic measures with an improved spray, for a while surpassed himself by an unbroken series of eighty successful cases. Yet, wonderful as this achievement was, it was only a difference in degree from his former experience, and assuredly no absolute proof of superiority of the new means employed. Of late I understand he has abandoned the spray for reasons which I had not the opportunity of hearing him explain when this subject was discussed at the Congress, and I am informed that he continues to have admirable results, which cannot surprise us when we remember what he had arrived at in his early period without adopting antiseptic measures. [It is important to bear in mind that Dr. Keith always paid the most scrupulous attention to cleanliness; and among the elements of that cleanliness he included what was a very important antiseptic precaution—namely, the purification of the sponges which he used by boiling them; impure sponges being undoubtedly a very fertile source of septic contamination, while protracted boiling is a most effectual means of destroying septic ferments.]

But how, it may naturally be asked, can the success of ovariotomy performed without the use of antiseptic means be reconciled with the truth of the antiseptic principle? The answer is, I believe, to be found partly in certain peculiarities of the abdominal cavity, and partly in circumstances common to wounds in general. One great peculiarity of operation wounds within the abdomen, as compared with those in ordinary situations, is that already referred to—viz., that the plasma from the cut surface is poured out into a large cavity lined with a serous membrane disposed to absorb it as fast as it is effused. Thus without drainage or any outlet whatever for discharge being provided, the serum is, under favourable circumstances, prevented from accumulating as it would in ordinary wounds similarly treated, and opportunity is not afforded for putrefaction.

Another favourable result of the disposition of the parts is that even if some accumulation of fluid does take place, the large size of the cavity, naturally adapted for variations in capacity, prevents the occurrence of tension, which is so common a cause of disturbance in ordinary wounds. More especially if a large tumour has been removed, the part affected is left in a state of the most perfect flaccidity and relaxation.

A further peculiarity in favour of abdominal wounds is due to the high vital power of the peritoneum. I recollect making a post-mortem examination in a case of strangulated hernia, where death had taken place within forty-eight hours of the operation, and finding it impossible to discover the site of the incision by inspection from within the abdomen, so completely had the peritoneal wound already cicatrised. This high degree of vital energy operates beneficially in a manner which I shall be better able to explain when I have spoken of some circumstances common to all wounds in their relations to septic agencies.

At the Cambridge meeting of the British Medical Association last year, I brought forward facts which showed that the serum of blood is not at all so favourable a soil for the growth of micro-organisms as I had previously imagined. If we take a glass of uncontaminated milk or urine, so arranged that if left untouched it will remain for any length of time free from organisms, and add to it a drop of ordinary water, we are sure to find in a few days evidence of bacteric development in the liquid. Indeed, in the case of milk, which appears to afford pabulum for almost all varieties of micro-organisms, I have shown that if a dozen glasses of the liquid in a state of purity, in vessels suitably arranged to prevent contamination from without, receive each one-hundredth of a minim of tap water, most of the glasses will develop bacteria, though of different species in the different vessels, showing how numerous and how various are the micro-organisms really present in water. Even the comparatively crude liquid which we call "Pasteur's solution," a mere solution of cane-sugar, tartrate of ammonia, and earthy salts, in which many kinds of bacteria refuse to grow at all, will be pretty sure to produce such organisms in a few days if a drop of tap water is added to it. Now from these analogies, and knowing as we do to our cost that blood serum is but too liable to putrefactive fermentation, I had assumed that ordinary water contained putrefactive bacteria in a form that would develop in serum.

But when, in the course of an experiment to be again referred to, I drew blood, with antiseptic precautions, from the jugular vein of an ox into a series of purified bottles, about half an ounce into each, and, having allowed the blood to coagulate and the clot to shrink, introduced various quantities of tap water to mingle with the expressed serum in the several vessels, I found to my surprise that not only an entire minim, but, two, four, and even eight minims, failed to induce putrefraction, although the bottles were kept in a warm box at the temperature of the body. I have since confirmed this experiment in the ox, and have also extended it to the blood of other animals—the donkey and the dog—with similar results. I even found that putrid blood in full activity, if largely diluted with water purified by boiling, and introduced in small quantity in proportion to the serum, failed to occasion putrefaction or the development of any organisms that I could discover by ordinary microscopic examination. Yet the same quantities of the same dilutions quickly gave rise to putrefaction in the blood of the same animal altered by mixing it with an equal part of purified water, showing that they really possessed septic energy, though unable to exert it upon normal serum. Not that the blood of these animals was in its natural state incapable of putrefaction, for inoculation with a very small quantity of undiluted putrid blood soon rendered it highly offensive. But the results of these experiments seemed to point to the remarkable conclusion that, after having been widely diffused by means of water, bacteria are incapable of developing in undiluted healthy serum. In this respect serum is totally different in its behaviour from milk, in which, as I have shown elsewhere, a single *Bacterium lactis*, detached from others by a similar process of diffusion by means of water, is as sure to produce its kind as are a million taken directly from souring milk.

How it is that the diffusion of bacteria renders them incapable of developing in serum I do not profess to understand. It may perhaps be that when bacteria are introduced directly from putrid blood, the products of the putrid fermentation adhering to them may induce chemically an alteration in the normal quality of the serum which, when thus impaired, may prove amenable to the nutritive energies of the micro-organisms, while conversely copious ablution with water may remove from the bacteria the associated substances which thus act as their pioneers. This view might be otherwise expressed by saying that the bacteria *per se* are unable to grow in normal serum, and can only develop in that liquid when it has been vitiated, whether by the addition of water or by the action of small quantities of the acrid products of putrefaction.

Or, again, it seems to me conceivable that the normal serum may oppose an insuperable obstacle to the nutritive attractions of an individual bacterium, but that this may be overcome by the associated action of several of the organisms in close proximity; after the analogy of the more energetic operation of a concentrated solution of a chemical reagent. In this case the diffusion by water would produce its effect by simply detaching and separating the bacteria from each other. But whatever be the explanation, the fact remains.

I have also made observations on the effects of exposure of uncontaminated blood to air in different localities, and I have found that even the introduction of considerable quantities of dust has not led to putrefactive change.

Applying this knowledge to the discussion of ovariotomy performed without antiseptic precautions, the question naturally suggests itself whether in many cases any septic organisms have really been introduced into the peritoneal cavity, either from air or from water, in a condition capable of developing in the effused serum. And thus we have suggested to us a further explanation of the success of such operations.

But the facts which have been elicited by the experiments referred to have a far wider range of application than to the special case of ovariotomy. They seem to indicate that the putrefaction so apt to occur in wounds not treated antiseptically is due rather to septic matter in a concentrated form than to the diffused condition in which it exists either in water or in air. They suggest the highly important question, Is the spray really necessary? In other words, Is there sufficient chance of the air of an operating theatre or private room containing septic matter which can prove effective in blood serum to make it needful to regard the question of contamination from the atmosphere at all? If the answer must be given in the affirmative, and the choice must lie between the spray and antiseptic irrigation during the operation at intervals varying according to the discretion of the surgeon, with syringing of the cavity of the wound after stitching, and syringing also at every dressing, then I should give my voice decidedly in favour of the spray, as being more sure of attaining its object and involving less irritation of the wound, and also (if carbolic acid be the antiseptic used) much less risk of carbolic poisoning. At the same time it must be distinctly borne in mind that the spray is, beyond all question, the least important of our antiseptic means, and that the circumstance that a surgeon does not happen to have a spray-producer at hand is no excuse whatever for his abandoning the attempt to obtain aseptic results. But if the apparatus for the spray is at my disposal, I for my part do not as yet dare to abandon it. By the careful use of our present means, the spray included, we have arrived, I think I may venture to say, at absolute security of attaining the great object in view, provided that we have the two essential conditions complied with: an unbroken skin to start with, and the seat of operation sufficiently distant from any source of putrefaction to admit of adequate overlapping of the surrounding integument by the requisite dressing. I leave it to those who have done me the honour to visit my wards to judge whether I am guilty of exaggeration in making this strong statement. Such being the case, I should not feel justified, except on perfectly established grounds, in omitting any part of the machinery by which results so important to our fellow-creatures have been arrived at.

Nevertheless I am aware that, concomitantly with the perfecting of the spray, there has been an improvement in other parts of our antiseptic arrangements, and I am not prepared to say that our increased uniformity of good results may not be due to the latter rather than to the former. And it may be, for aught I know, that when the International Medical Congress next meets I shall be able to speak of results of a still higher order obtained without using the spray at all. For if further investigation should confirm the conclusion to which our recent facts seem to point, and it should indeed be proved that all idea of atmospheric contamination of our wounds during operations may be thrown to the winds, then no one will say with more joy than myself, "Fort mit dem Spray."

The fact that normal serum is not made to putrefy by the addition of a small quantity of water came out unexpectedly in the course of an experiment designed to illustrate a view which I had long entertained as an inference from circumstances observed in surgical practice—viz., that an undisturbed blood-clot has a special power of preventing the development of septic bacteria. This property I ascribed to the white corpuscles, which are well known to retain their life long after blood has been shed from the body. I supposed these living elements of the clot to produce this effect in accordance with a principle which I believe I happened to be the first to demonstrate, but which is now generally admitted—viz., that the tissues of a healthy living body have a power of counteracting the energies of bacteria in their vicinity and preventing their development.

The experiment which I related at Cambridge seemed to confirm this view completely. I have already described one part of that experiment, consisting of the introduction of small quantities of water into vessels containing uncontaminated blood after coagulation had occurred, and the clot had shrunk so as to press out the serum. In the other part of the experiment the water was introduced before the blood into the purified bottles which were to receive it, so that any organisms which the water contained were diffused in the entire mass of the blood before coagulation, and were presumably retained in the clot when it shrank and pressed out the serum. And unexpectedly great as the resisting power of the serum to the development of micro-organisms proved to be, that of the coagulum seemed markedly greater. But our knowledge of the remarkable influence of the quantity of the ferment in proportion to the serum, as indicated by the more recent experiments, makes the evidence on which I relied at the Cambridge meeting far from conclusive, so that I cannot now regard it as demonstrated for blood outside the body that the coagulum has a greater power than the serum of resisting putrefaction. But what I desire particularly to mention on the present occasion is an experiment with reference to the behaviour of the coagulum under the circumstances in which it especially interests us as surgeons—viz., within the living body. The experiment was of a character such as it would have been difficult under existing circumstances to perform in London, so I resorted to the Ecole Vétérinaire of Toulouse, where everything was most liberally placed at my disposal by my friend Professor Toussaint and others in authority at the institution.

Having provided four pieces of glass tube, each about an inch and a half in length and three-hundredths of an inch in diameter, containing two pieces of silver wire twisted together, with a little piece of fine linen cloth fixed between them at their central part, the whole apparatus having been purified by steeping in a strong watery solution of carbolic acid, and subsequent boiling in water and drying over a lamp, I applied to each bit of linen one-twentieth of a minim of septic liquid, which was in the case of one of the tubes undiluted putrid blood, in the second that blood diluted with ten parts of pure (boiled) water, in the third a dilution with a hundred parts of the water, and in the fourth a thousandfold dilution. I then exposed the jugular vein of a donkey in five places, with antiseptic precautions, leaving four intervening portions of the vessel intact and covered with the integument, and introduced into each undisturbed portion of vein one of the glass tubes with the septic rag in its interior, strong ligatures of string purified with carbolic solution being applied tightly as each tube was introduced, so as to form four venous compartments or capsules quite distinct from one another, and consisting of vascular tissue healthy at the outset.

After three days the animal was killed, and the venous compartments examined. In all four putrefaction had occurred within the glass tube; but the state of the coagulum outside the tube was extremely interesting. I must content myself on the present occasion with referring to the condition of the first compartment, or that which had received the undiluted putrid blood. Immediately around the glass tube the clot was so much softened by putrefaction as to be almost diffluent. But next to the greatly thickened wall of the vein, and forming an adherent lining to it, was a layer of firm coagulum, in some parts about a quarter of an inch thick, resembling to the naked eye a perfectly recent clot. Before introducing the tubes into the vein I had drawn blood from it, with antiseptic precautions, into a number of purified stoppered bottles containing each the same quantity of septic liquid as the tubes received ($\frac{1}{20}$ minim) of various degrees of dilution, from the unmixed putrid blood to a mixture with 100,000 parts of boiled water. The bottles received each about as much blood as a venous compartment contained, and they were kept at the temperature of the body. Three out of four which had received the 100,000 fold dilution as well as two kept uninoculated as standards of comparison, remained permanently free from putrefaction, but those inoculated with larger proportions of the septic material putrified, and those which received the undiluted putrid blood, were, at the time when the vein was examined, in a state of utter decomposition, presenting a very striking contrast with the apparently unchanged layer next the wall of the vein.

But unaltered as this layer appeared to the naked eye, the microscope showed it to be in reality a very different structure from a recent clot. It proved to be teeming with cells of new formation, far more numerous than the white corpuscles of the blood, and differing from them altogether in characters; being as a rule larger, and often of great size with pellucid contents, and having as their nuclei bodies more or less closely resembling pus corpuscles. On the day after the death of the animal and removal of the vein, I continued the examination of the first compartment; and I was tearing off a part of the firm coagulum from the lining membrane when I opened into a little cavity in the clot about a quarter of an inch long and one-eighth of an inch in other dimensions, containing a thick liquid of pearly white colour, the cavity having a thin grey lining separated by a layer of dark clot from the wall of the vein, except at one part. Here it communicated with a small venous branch, whose contents had been entirely converted into this white liquid for a short distance, beyond which the branch was obstructed by clot. On microscopic examination I found this white liquid composed entirely of closely packed corpuscles of new formation, like those above described as infiltrating the clot. Here, however, the new cell development had taken place at the expense of all the original constituents of the coagulum, so that the fibrin had entirely disappeared, and only a stray red corpuscle here and there was to be discovered, and no granular débris was observed. The liquid was, in fact, neither more nor less than pus, and the cavity a small abscess. The evidence of endogenous cell development was in this liquid extremely striking. Many of the corpuscles resembled those of ordinary pus, though of varying dimensions; but often bodies exactly similar to the free pus corpuscles were seen still included as nuclei within large pellucid cells. In one such cell which I sketched there were four nuclei, three of which exactly resembled the free pus corpuscles. Thus I had the opportunity of repeating observations made in the course of very similar experiments carried on as early as 1864, experiments which, I believe, helped to prepare my mind for applying to surgical practice the conclusions of Pasteur as to the nature of putrefaction. Those observations have never yet been published, as I was compelled to suspend the investigation by the pressure of clinical work in connexion with the development of the antiseptic system. They proved, however, in the clearest manner that a blood-clot within a vein is, under septic influence, liable to a genuine suppuration—that is to say, to a change which is no mere result of breaking down of fibrin and accumulation of white corpuscles of the blood, but consists of a growth of living corpuscles multiplying by endogenous cell development at the expense of the original constituents of the coagulum.

As to the source of these newly formed corpuscles, we must suppose them to have been derived either from the white corpuscles of the blood or from proliferation of elements of the tissues of the wall of the vein. If we conceive them to have originated in white corpuscles of the blood, it is nevertheless quite certain that they were not mere emigrated white corpuscles, but that they had sprung from them as a new and altered progeny. The actual appearances presented favoured the view that the new cells had been derived from the tissues. On examining stained sections of the wall of the vessel and the adherent coagulum, I found that the corpuscles in the latter were similar in character to those which thronged the interstices of the tissue of the inner part of the vascular wall; and among the latter were some that conveyed the idea of transition from the normal tissue elements. And this view seems confirmed by the fact that I have never succeeded in obtaining any new growth of corpuscles in coagulum outside the body, although the blood has been subjected to very various degrees of septic agency and has been kept in conditions similar to those within the body as regards temperature and moisture, and, indeed, in every respect, so far as I can judge, except only the influence of surrounding living tissues.

There was another circumstance observed in the clot within the vein which I must not omit to mention. Even the parts next to the lining membrane, though not putrefying, had a distinct faint putrefactive odour. This is readily explained on physical principles by diffusion of the products of the putrefactive fermentation which was going on within the tube and in its vicinity. Indeed, one of the other venous compartments, on being opened, gave exit to a small amount of fetid gas, the remains of that which had been evolved from the putrefying part, and which had been only partially taken up by the surrounding parts; but in the other compartments it had been all so disposed of. This diffusion of chemically irritating products of putrefaction beyond the limits of the actual septic process seems to me a matter of much importance; and in the case of the ass's jugular it appears to explain, what otherwise might be difficult to account for, the inflammatory thickening of the coats of the vein, and also of the surrounding tissues. For the striking contrast presented between the inflammatory changes in and around the vessel in this animal and the entire absence of such appearances which I have found both in the horse and in the calf where I have tied the carotid artery antiseptically, with silk in the one case and with catgut in the other, seems to imply that the septic matter introduced within the vein of the donkey was the cause of the disturbance in question; and the mode of its operation would seem to have been that above indicated.

But to return from this apparent degression. I have to add that, although twenty-four hours had passed since the vein was taken from the animal, I saw, in the course of a prolonged investigation of the pus in the little abscess, only a single bacterium. Also I have to mention the important fact that in the examination of the previous day, when the vein had been just taken from the body immediately after death, whereas in the softened part of the clot in the vicinity of the glass tube the red corpuscles were utterly altered, and accompanied by abundant bacteria of different kinds, some in active movement, the piece of firm clot from near the wall of the vein showed the red corpuscles often exactly like those of freshly drawn blood; while, if any bacteria were present, they did not declare themselves to ordinary microscopic examination.

Thus the experiment affords very striking proof of the power of an undisturbed healthy coagulum in the vicinity of living tissues to resist the development of putrefactive bacteria, even when present in a highly concentrated form, while we have, as I believe, an explanation of this power in the multitude of new living elements with which the clot was peopled.

Turning now to the application of these pathological facts to ovariotomy, we find that they seem to complete the explanation of the success which has attended that operation performed without antiseptic precautions. Among the points in Dr. Keith's earlier practice which most excited alike my admiration and my astonishment was the way in which he diagnosed an accumulation of fluid in the pouch of the peritoneum termed Douglas's space, and, drawing off by puncture per rectum with cannula and trocar a quantity of putrid liquid, saved his patient from impending death. I greatly admired the skill of this practice; but I was astonished at the pathological facts. How was the septic process limited and prevented from spreading throughout the cavity of the peritoneum? This seems now to admit of interpretation. In such cases septic matter must have been introduced during the operation in a form capable of developing in the blood; and if we suppose a portion of such effective septic ferment imprisoned in the interior of a coagulum in the space referred to, this clot, situated within the living body, may be conceived to oppose an obstacle to the development of the putrefactive bacteria in its substance, as was the case in the venous compartment of the ass's jugular. And here the high vital energy of the peritoneum would come into play. We have seen how rapidly a wound in the peritoneum may heal, and this rapid healing is, in other words, the rapid peopling of the lymph in the wound with vigorous new living elements. For, whatever view may be taken as to the source of these new corpuscles, certain it is that the organisation of lymph proceeds more rapidly in proportion as the wounded tissues are in a more vigorous condition. Thus a wound on the dorsum of the foot of a feeble old man, though strictly protected from septic influence, heals very languidly; while a cut in the lip of a child unites with great rapidity. This, indeed, would seem a further argument in favour of the tissues being the source of the new corpuscles. But be this as it may, the lymph on the peritoneum becomes very rapidly thronged with new living elements, and the same will be the case with a blood-clot, which differs from recent lymph merely in containing the additional and probably entirely neutral element of the red corpuscles. But in the meantime, as, in the ass's jugular, the tissues of the vessel were irritated, as it would appear, by the acrid products of putrefaction diffused around the septic parts of the clot, and penetrating to those which were not really septic, so would the parts of the peritoneum surrounding the clot in Douglas's space be excited to adhesive inflammation, gluing the opposed surfaces together by lymph; and again this lymph, becoming rapidly organised through the high vital energy of the peritoneum, would oppose an effectual barrier to the spreading of bacteric development should it extend to the surface of the clot. At the same time the irritation caused by the putrid products, where most intense, would induce copious effusion of plasma and accumulation of putrid serum. In time this state of things would lead to limited abscess, such as has been occasionally met with after ovariotomy, the discharge of the putrid pus having in some instances been followed by the recovery of the patient.

But the power of organising blood-clot or lymph to resist the advancing development of putrefactive bacteria has, like the behaviour of the serum in relation to putrefaction, a far wider range of application than to the explanation of the success of ovariotomy performed without antiseptic treatment. It serves, for example, as I believe, to explain a fact like the following:—In Mr. Syme's celebrated case of diffuse aneurism of the axillary artery, treated by "the old operation," the aneurismal clots had not only enormously distended the axilla, but reached backwards so as to raise the scapula, and upwards high above the clavicle. He entrusted to me the duty of compressing the subclavian artery; and, as a preliminary measure, made a small incision penetrating through the deep cervical fascia, so as to enable my thumb to be thrust through the mass of clots in that situation, and reach the first rib, on which the vessel lay. He then laid open the mass in the axilla by a free and rapid incision, and scooped out with his hands enough of the clots to enable him to gain access to the arterial trunk, and tie it above and below the place of its communication with the aneurismal cavity. Making no attempt to remove the rest of the coagula, which indeed would have been quite impracticable, he simply brought the cutaneous margins of the wound together by interrupted sutures, the ends of the silk ligatures being left hanging out of the wound, in accordance with the then universal practice; and as this was before the days of antiseptic management, no special treatment of that kind was of course employed, but a dressing of dry lint for the first few days, and water-dressing afterwards. Considering the well-known danger of diffuse suppuration, which without antiseptic treatment used to attend free incision into a mass of extravasated blood, I was astonished to see both the puncture above the collar-bone and also the main wound heal in the kindest manner, without more suppuration from the latter than must have attended a wound made in healthy tissues as a consequence of the presence of the ligatures. The fact now ceases to be wonderful if we take into account the circumstance that the outlying clots had been for several days among the tissues, and had thus naturally acquired new living elements which would confer upon them the power of fencing themselves against advancing putrefaction.

Our principle seems, however, to find its widest application in aiding to explain the possibility of union by the first intention without the use of any antiseptic means, nay, in spite of the application of septic ones. For such is, in truth, the apparently cleanly water-dressing. As certainly as we remove it from a wound after the lapse of twenty-four hours, do we find that the diluted blood serum which then soaks the lint has a putrid smell, implying that it contains septic ferments such as would assuredly act effectively upon blood outside the body. And yet, in the not uncommon case of the occurrence of primary union under such treatment, putrefaction fails to spread into the wound; for if it did so it would inevitably provoke suppuration.

Long before I entertained the idea of the antiseptic principle I often contemplated with wonder the behaviour of a thin layer of lymph or coagulum between the surfaces of a healthy wound, as contrasted with that which the same material would have exhibited, had it been placed in similar conditions as to temperature and moisture, between two sheets of glass or gutta-percha. The absence of putrefaction in the former case, as compared with its occurrence in the latter, was plainly due in some way or other to the influence of the living tissues, between which the putrescible lymph or blood-clot lay. Beyond this, however, all was mystery. Afterwards when the power of healthy living tissues to oppose bacteric development became apparent to me, as exemplified by the inability of bacteria to grow in the mucus of a healthy urethra, I attributed the absence of putrefaction in the healthy wound to a direct control on the part of its tissues over the septic bacteria. There is, however, a defect in this explanation—viz., that, whatever be the nature of the controlling agency of the tissues on the bacteria in their vicinity, it can hardly be conceived that it is exerted beyond an extremely limited range; so that in order that it should be effective, a more accurate and close apposition of the cut surfaces would seem necessary than actual experience shows to be required. And here our new principle comes to our aid, when we learn that the clot itself, as it becomes organised, acquires the defensive property which remained to be accounted for.

The thinner the layer of the clot, the more rapidly, *cœteris paribus*, will it be densely peopled throughout with the new corpuscles, which confer upon it this defensive power; and the process will also advance quickly in proportion as the tissues of the cut surfaces are vigorous and active. And thus we have the *rationale* of the rules of the older surgery in aiming at union by the first intention—viz., to perform the operation as much as possible by clean cuts with a sharp knife that shall injure the tissues as little as may be, apply the cut surfaces closely together, at the same time providing an outlet for effused blood and serum, and afterwards endeavour to avoid inflammatory disturbance, which in proportion to the degree in which it exists, enfeebles the vital powers of the part affected.

Yet sound as these maxims were in the then existing state of surgical science, the best efforts of the surgeon were too often thwarted by "unhealthy actions," as he termed them, of which he did not understand the cause, and which he was consequently powerless to prevent. And we must not allow an exceptional case like ovariotomy to blind our eyes to the truth that the disasters of surgery in the past have been essentially caused by septic agencies. Simplification of our means of procedure is no doubt in itself highly desirable, and I have already indicated one direction in which it may possibly be attained. But the safety of our patients incomparably transcends such a consideration, and it would indeed be a grievous thing if our desire for simplicity should induce us in any degree to relax our efforts to carry out the strict antiseptic principle, the strenuous endeavour so to deal with wounds as to prevent from first to last the development in them of pathogenic organisms. The means by which this object may be most surely and at the same time most conveniently attained will, no doubt, vary greatly in the future in accordance with our ever-advancing science; but whatever modifications we may admit in our methods let us at all events never be satisfied with any that does not yield results at least as good as those which it is now in our power to secure.

If we suffer ourselves to be drawn aside from the strict antiseptic principle, we shall not only subject our patients to the risk of the old disasters, but we shall be compelled to withhold from them the benefit of valuable procedures which strict antiseptic management alone can warrant. Take as a single example the case of a loose cartilage in the knee-joint. To remove it by free incision is the most simple and satisfactory treatment, except for the attendant danger which was formerly so great as to be prohibitory, but of which our present means of carrying out the antiseptic principle have entirely disarmed it. If such a procedure was ever ventured on without antiseptic means, the only chance of success lay in accurate closure of the wound with a view to primary union. On the other hand, under antiseptic management, I systematically abstain from closing the wound completely, leaving a part unstitched for the introduction of a drainage-tube, so as to guard against the inflammatory disturbance which might otherwise result from accumulation of fluid in the articular cavity. In other words, I abstain from the only means which would have afforded hope of success without antiseptic treatment, and I adopt means which, without antiseptic treatment, must infallibly lead to disaster through septic suppuration of the articulation. Here, then, I conceive we have a true touchstone of the truth of the antiseptic principle. The loose cartilage which I hold in my hand was removed from the knee of a gentleman a fortnight ago. It lay imprisoned in the angle between the anterior part of the articular surface of the tibia and the femur. I extracted it by free incision; and as the situation was one which did not admit of convenient insertion of a drainage-tube into the joint, I left the wound widely gaping throughout. That patient has not since experienced the slightest uneasiness, nor has there been any disturbance, local or constitutional.

And this leads me to make the general remark that under strict antiseptic treatment union by first intention has no longer the importance it used to possess. As regards the essential points of avoidance of inflammation and fever, of pain and danger, it is a matter of absolute indifference whether primary union occurs or not. Nay more, as in the case just referred to, if we wish to make doubly sure of preventing all inflammatory disturbance, it is sometimes well to avoid stitching, and the tension which may become associated with it. There is at present in King's College Hospital a man on whom I operated three weeks ago for ununited fracture of the humerus, cutting down on the fragments and uniting them by a suture of thick silver wire after sawing off their extremities. Now, in a case of this kind some years ago in which I applied stitches to the wound, although a good-sized drainage-tube was used, a certain amount of inflammatory disturbance occurred, which I could only attribute to want of sufficient exit for the abundant sanguineous oozing that took place from the osseous surfaces. This disturbance led not merely to some deep-seated suppuration, but to necrosis of a portion of one of the fragments, which greatly retarded the cure. Hence I have since abstained from anything like close stitching after such operations, and in the patient referred to, the wound was left gaping widely. At the next dressing it was found occupied by blood-clot, which has subsequently become organised, and at the same time has contracted so much that the cicatrix, already nearly complete, promises to be little more than linear. Meanwhile there has been no uneasiness, redness, or swelling, and no febrile disturbance, and if any imperfect pus whatever has been formed, it has been only from the surface of the organised coagulum as a result of slight unavoidable stimulation by the antiseptic used in the dressing. And here we have, as I believe, another touchstone of the antiseptic principle, showing that it is true, and gives results both new and important. For without effective antiseptic means of some kind or other, such a course of such a wound would, I believe, be impossible. The impermeable protective layer applied next the wound under the antiseptic gauze prevented evaporation, and maintained a constant state of moisture of the surface of the clot. Now, a moist dressing other than an antiseptic one could not have failed to occasion putrefaction of the exposed coagulum and suppuration of the cavity of the wound. Yet, all who are familiar with antiseptic practice know that it is a common thing to see the organisation of the blood-clot proceed under the protection of the superficial layer of coagulum to complete healing without a particle of suppuration, a scar being found when the superficial layer is detached.

Time would not permit me to refer to all that has been communicated by those who have taken part in this discussion. I cannot, however, forbear making a passing allusion to the extremely remarkable results which have been related by Professor Esmarch as obtained by his permanent dressing—results so surprising that they would be incredible were it not for the perfect trustworthiness of the authority that vouches for them. And I would ask those who advocate mere cleanliness, as distinguished from antiseptic practice, how they can reconcile their views with facts such as these? What can be more dirty, in the ordinary acceptation of the term, than a wound left covered up with the same dressing for weeks together, the original blood and serum remaining upon it intact under this "Dauer-Verband?" Yet it is surgically clean because it is aseptic. On the other hand, the æsthetically cleanly water-dressing is surgically dirty, because it contains elements which give rise to septic changes in wounds.

One other point which has been referred to in connexion with this debate is of so much importance that I cannot but notice it. The old objection has been revived that antiseptic treatment leads the surgeon to concentrate his attention upon local measures, to the neglect of general hygienic arrangements and a due consideration for the constitutional state of the patient before subjecting him to operation. I do not think that this charge is at all warranted by facts. For my own part, I have from the first used antiseptic treatment even for superficial wounds and sores, not so much for the sake of the individual cases (which I knew did well, as a rule, under water-dressing) as for the express purpose of preventing them from contributing elements of general unhealthiness to the hospital ward. And if I have sometimes allowed the beds under my charge to be more crowded than is in accordance with the views of modern hygiene, it has been only after such a condition, originally brought about by accidental circumstances, had proved as a matter of fact consistent with perfect healthiness of the patients. The septic element being suppressed, less space was found to be essential to salubrity.

As to the other part of the charge—viz., that antiseptic treatment leads to a less careful selection of patients for operation than ought to be made, it seems to me to be refuted by the success which confessedly attends the practice. If, indeed, the charge were well founded, and we did really operate upon patients whose constitutions rendered them unfit subjects for such interference, good general results obtained under circumstances so highly unfavourable would afford the most conclusive proof of the value of the local treatment employed. But the truth is, that the suppression of the septic element enlarges the capabilities of surgery in the constitutional direction no less than in the local; and enables us to extend the benefits of needed operations to patients whose constitutions are so enfeebled by age or vitiated by disease that without strict antiseptic treatment no prudent surgeon would venture to meddle with them.

I appeal to the logical faculty of this great assembly of eminent men, and beg them to consider carefully in relation to this question the familiar case of a simple fracture or dislocation. Do we feel anxiety regarding the state of the constitution of a patient who has received such an injury? The mischief done is in itself of a worse character than the surgeon ever inflicts. Yet so long as the unbroken skin shields the bruised and lacerated tissues from the access of materials coming from the external world, repair advances safely, no matter what be the constitutional condition; the exceptions being so extremely rare that we practically leave them out of consideration altogether. It therefore surely follows that if we could contrive a treatment of our wounds which would have all the advantages of the unbroken integument, we might operate without anxiety on account of the constitution.

To provide a condition of our operation wounds that shall put them fully on a par with subcutaneous injuries is plainly the ideal of our art. Towards the attainment of this ideal we have already made large progress; and towards its full achievement, so far as it be possible, I would earnestly invite the best efforts of my hearers.—*Lancet, Nov.* 19, 26, 1881, *pp.* 863, 901.

34.—MODIFIED LISTERISM—THE LATE MR. CALLENDER'S METHOD OF WOUND-DRESSING.

By Frank M. Pope, M.B.Cantab., Leicester.

Mr. Turton, of Heckmondwike, in his letter in your issue of the 1st inst., recommends the use of carbolised oil, strength 1 in 20, with a covering of carbolised tow as a modification of Listerism, and as an efficient wound dressing.

As an old dresser of the late Mr. Callender, I wish to state that the use of any other dressing in his wards was exceptional. A piece of gutta-percha tissue was usually placed between the oiled lint and the tow or "marine lint." I desire not so much to point out the claim of the above most eminent surgeon to be the greatest exponent of this system of dressing wounds—as that will readily be allowed by all old Bartholomew's men, at least of the last ten years, as well as by most well-informed surgeons—as to draw attention to the fact, which he was always anxious to impress on his pupils as well by his practice as his words, that carbolic oil becomes all but inert as an antiseptic dressing after being kept a few hours. His method was to have carbolic acid and olive oil, side by side in the ward, to be mixed in their right proportion (in his wards about 1 to 12), a few minutes only before the dressing was applied. His cases almost always did well, as his statistics in the hospital reports will show.

I think the disuse of carbolised oil dressing in hospitals is mainly due to the fact that the mixture is sent up to the wards in large bottles from the dispensary, and kept there till used up, sometimes for weeks or even months. If any person will compare the appearance and smell, especially the latter, of a freshly made sample, with those of one that has been kept for a month or more, he will at once be aware of a great difference; and if he will further make a trial, by treating similar cases with the two sorts, he will be convinced of the deterioration of the mixture in antiseptic qualities after being kept.

Mr. Callender was fully persuaded that a change in composition took place in the carbolised oil in a comparatively short time, though I have heard him state that he was not able to explain it on chemical grounds. I may add, that I have mentioned the facts to more than one noted chemist without being able to obtain any suggestion as to the decomposition or reaction, if any, which takes place. If any of your readers can supply such a one they will, I am sure, confer a benefit on surgery.

I am not aware that this point has before been discussed. If this method of dressing wounds can be reinstated in the good opinion of the profession, it will prove a great boon to the general practitioner, as all the appliance needed for carrying it out is a small phial of liquefied carbolic acid, carried in the pocket, as the oil can be found in the remotest hamlet known to country practice. I may add, that Mr. Callender, on uncovering a wound, always had ready a camel's hair pencil, dipped in the prepared oil, and with this lightly brushed it over immediately on removing the dressing, thus forming an antiseptic film, effectually protecting the surface from the contact of air-borne germs.—*Lancet, April* 15, 1882, *p.* 626.

35.—ON EXCISION OF THE KNEE-JOINT.

By PATRICK J. HAYES, F.R.C.S.E., Surgeon to the Mater Misericordiæ Hospital, Dublin.

The steps of the operation for exsection of the knee are too well known to render any lengthened account of the procedure needful. I usually flex the leg moderately, and, having defined the posterior margins of the femoral condyles, I cut from one of them to the other, straight across the ligamentum patellæ, and into the joint. At this stage the patella may be dissected from its attachments, but if fixed to the femur its separation will probably be postponed until after division of fibrous connections between the femur and tibia. I prefer cutting through the lateral ligaments before proceeding to the division of either the normal or abnormal structures occupying an intra-articular position; also, when dealing with the latter, I keep the leg strongly flexed, and direct the cutting edge of the knife against the articular surface of the head of the tibia rather than towards the ligamentum posticum. I never attempt to clear the posterior aspect of either the femur or the tibia before applying the saw. I sever the bone from before backwards, and break through the posterior surface of each bone. This is done with the view to avoid injuring the posterior ligament and corresponding fibrous connections between the femur and tibia, it being an advantage to preserve, if possible, these tissues, as they not only assist to maintain contact between the sawn surfaces, but also, should suppuration occur in spite of antiseptic dressings, they will in all probability prove a barrier against the entrance and burrowing of pus into the popliteal space.

As the *raison d'être* of this communication refers to my method of putting up the limb, I shall describe in detail the steps to be adopted. Presuming that the operation will have been performed under an antiseptic spray, the spray should now be directed across the region of the knee, so as to avoid any unnecessary wetting of splint, bandages, &c. Carbolised sponges are to be maintained in contact with the angles of the wound, so as to absorb all blood-flow whilst the leg and thigh are being washed and bandaged.

The surgeon next applies a soft flannel roller evenly, but loosely, around the limb from the toes to a point about two inches below the inferior lip of the operation-wound, and over this a second roller is to be adjusted, thus providing the leg with a sufficiently thick and soft covering. In like manner the thigh from the groin to a point about two inches above the wound is to be loosely encased with a couple of flannel rollers. The limb being ready for application of the splint, the patient is to be brought *thoroughly* under the influence of ether, so as to produce complete relaxation of the muscles. The splint consists of two concave pieces of perforated iron — the one moulded so as to fit the posterior aspect of the leg, and the other adapted to receive the posterior surface of the thigh— connected posteriorly by means of a strong, flat, but narrow, bar of iron, so bent as to form an oblique step about three inches long, and having the end to which the leg-piece is attached exactly one inch in advance of that fixed to the thigh-piece. This apparatus is to be provided with pads arranged for leg and thigh—the leg pad being made thicker below than above—and then it is to be carefully adjusted behind the limb. A soft pad is now to be laid in front of the thigh near its lower end, and upon this pad a concave piece of iron about four inches long, by from two and a half to three inches wide, is to be placed. Sometimes I lay a square of poroplastic substance larger than the concave plate of iron between the latter and the pad. The next step is to firmly secure the thigh in the upper part of the splint. This is done by encircling the splint and limb with a strong strap which is to be tightly buckled across the upper part of the anterior small splint, whilst lower down the strap of a Petit's tourniquet is to be fixed, the brass of the tourniquet resting against the anterior splint. As both straps will have been drawn extremely tight, it is clear that one or two turns of the tourniquet will firmly press back the lower end of the femur, so as to render the anterior surface of that bone flush with the anterior surface of the tibia.

Entrusting the upper part of the limb to his assistants, the surgeon next proceeds to encase the foot in several turns of a gypsum bandage, which is to be carried upwards, encircling the leg and lower part of the splint as high as the point at which the flannel bandage terminates. Whilst this is being accomplished the foot must be held at right angles to the leg, lest extension of the ankle should occur and prove a source of trouble at a later period. When the operator will have satisfied himself that the limb has been properly arranged and secured, the wound is to be closed, while drainage is to be provided for by having short, flanged tubes inserted, either at the angles of the original wound or through button-hole apertures which may be made still further back. After the application of antiseptic dressings the limb is to be swung by means of a loop of calico passed behind the leg-piece of the splint and tied to the bars of a strong and high fracture cradle. It will be seen that my apparatus forms a light, portable, but extremely secure, means for fixing the limb. A foot-piece is altogether unnecessary, as the gypsum bandage encircling the leg and splint constitutes a firm boot. Shortness of the thigh-piece, combined with suspension of the leg, enables the patient to assume the sitting posture and change position without risk or discomfort.

I have frequently encountered cases of articular disease where the morbid affection seemed limited to the soft structures, but where complete exposure of the bony surfaces disclosed the presence of localised caries and of suppuration in the osseous tissue. This fact alone would determine me against attempting to perform excision by making the comparatively small lateral incisions recommended by some surgeons. Moreover, an unwounded state of the integument in front of the knee cannot prove of any real advantage to the patient, as the rule, in my experience, has been for the central portion of the operation-wound to unite by the first intention, whilst at the time of putting up the limb the surgeon is enabled clearly to see as well as to feel, and of course to regulate, the respective positions of femur and tibia.

Whenever I have to deal with a cavity in the cancellous tissue of either the tibia or the femur, I scoop the space clear in the first instance, then by means of a small piece of fine sponge wetted with chloride of zinc solution (gr. 20, ad ℥ i.) I mop the cavity thoroughly—in many cases I have judged it necessary to drill the bone, so as to make a free counter-opening from the deepest part of the bone cavity in the track of the operation-wound. I never sponge a wound in the soft parts with chloride of zinc solution, for I have found the salt to cause a very considerable flow of blood-stained serum, calculated to prevent union by adhesion. I always arrest parenchymatous bleeding (which is sometimes inconvenient after the removal of Esmarch's bandage) by applying to the wound a succession of sponges squeezed out of *very* hot carbolic solution, after the method proposed by Dr. P. Browne. This proceeding will control oozing from the bone surfaces as well as from soft textures, and it in no way tends to interfere with the early development of repair.

I have never seen bleeding from a large nutrient artery in bone, but if the surgeon should encounter such, I think hemorrhage could be at once arrested by plugging the canal with a pointed piece of decalcified bone cut from one of Neuber's drainage tubes.—*Dublin Journal of Medical Science, Feb.* 1882, *p.* 84.

36.—ON FRACTURE OF THE PATELLA; THE CAUSE OF DISPLACEMENT OF THE FRAGMENTS, AND THE MEANS OF REMEDYING IT.

By Professor Jonathan Hutchinson, F.R.C.S., &c.

I hold definitely, respecting fracture of the patella, that the separation of the fragments is not caused by the muscles. Repeated observations have convinced me that it is always caused by, and in proportion with, the effusion into the joint. If there be no effusion, there is no separation. The muscle is not a piece of India-rubber to contract, and remain contracted, as soon as one end is loosened. It is as easily capable of relaxation as it is of contraction, and, when the limb is at rest, it is always relaxed. When relaxed, there is no reason why the upper fragment of the broken bone should not come easily down to the other; and, in point of fact, in cases where there is no effusion, it does do so. I have demonstrated this repeatedly. You may then clear your minds of all misconceptions as to spasm of the muscle being the cause of persisting separation. Such spasm may, of course, cause separation at the moment of the accident; but, as soon as the limb is at rest in bed, its agency ends. I repeat, you will always find the difficulty of approximation in proportion to the amount of swelling. This fact kept in mind, the means of treatment will be evident. Get rid of the effusion as quickly as possible. The effusion may be be of blood, or it may be of synovia, or perhaps, most commonly, of a mixture of the two. If it occur immediately after the injury, then it is probably blood; and these cases are the most difficult to treat, for blood is more slow of absorption than synovia. The treatment is, however, the same for both, and consists in the vigorous use of cold. The ice-bag sedulously applied, or a spirit lotion so freely used, that evaporation is constantly going on, are the best measures. You must not be content unless the skin over the whole part of the knee be kept quite cold. It is of great importance that absorption should be rapid and complete. If you can get rid of the swelling in a week or ten days, you will have a good chance of bony union, or of union so close that it is equivalent to bony. I believe that we get bony union in nearly half our cases. Our measures are, then, ice for a week or ten days; then oblique strips of plaster, which fix the fragments, and catch in notches in the splint. We avoid all complicated forms of apparatus; and hold, respecting Malgaigne's hooks and some similar contrivances, that they are barbarous and dangerous. The limb is, of course, always extended, from first to last, on a long and broad back splint, with a thick cushion and side notches. The limb is kept a little elevated—not with any design of relaxing muscle, but because the knee-joint permits slight overextension, which is attained by elevating the leg. By this means, the lower fragment passes a little higher up than it would otherwise do. When, at the end of ten days, you have brought the fragments together, cover the whole joint with bandages, and never touch the bone again until six weeks are accomplished. If you leave it uncovered, some zealous and inquiring dresser is almost sure to examine the bone to see if it be uniting, and, by such examination, he prevents the progress of union. You must leave it in faith, and even at the end of the six weeks must still be very careful, and on no account make lateral movements. The union is still weak, and will easily break down. Be content if you find the fragments close. At the end of six weeks or two months we usually allow the patient to get up, but he is always provided with a patellar apparatus before doing so. Note that the object of this apparatus is not to hold the fragments together, which it could not do, but to prevent flexion of the knee, which would drag away the lower fragment. It also serves a secondary purpose, of giving lateral support to the joint. If an apparatus be not at hand, a gum-and-chalk or plaster-of-Paris case will serve the purpose quite as well. I usually advise our patients to wear the apparatus for six months; and then, if the knee feel strong, to throw it aside. Although we aim at bony union, and, I believe, often obtain it, yet I am by no means an enthusiast as to its advantages. It curiously happens that those who have fibrous, or even ligamentous union, not unfrequently walk better than those in whom it is close. This I have proved by examination of many patients, at long intervals after the accident; and it is a very strong reason for declining to resort to any mode of treatment involving risk. A curious and unexpected result, to which I often ask your attention, is, that the quadriceps muscle usually undergoes, during the six weeks' rest, very decided atrophy, sometimes attended by a certain degree of contraction. This, in cases in which the bone is closely united, much limits the movements of the joint. I well recollect one man, whom we treated with great care, and in whose bone ossific union occurred. Two years later he fell and broke the same bone again. His surgeon, on the second occasion, was not so successful as I had been; and when, a year later, I had an opportunity of examining the joint, I found considerable separation, the union being only by ligament. To my chagrin, however, the man stated that the second accident had enabled him to walk much more easily, for, during the interval between the two accidents, his knee had always felt stiff and awkward.—*British Medical Journal, March* 4, 1882, *p*. 298.

37.—ON THE REDUCTION OF DISLOCATIONS BY MANIPULATION.

By J. H. **Anderson**, M.D., Surgeon, Royal Navy.

In the Lancet of May 7th, 1881, I read with much interest a letter by Mr. C. H. Greene of London, on "Dislocation of the Shoulder-Joint, and Death from Chloroform," in which he recommended a plan for the reduction of dislocations, very similar to one which was first suggested to me by Professor Gordon, of Belfast, and which I invariably practised during the years 1869-71, when I held the appointment of house surgeon to a general hospital in a large manufacturing town in the north of Ireland, where all the serious accidents—and they were not a few—occurring among the working classes, were brought for treatment; and among them dislocations were of frequent occurrence.

I can remember only one instance in which the plan I refer to failed, and that in a very muscular man in whom the luxation—of the shoulder—was of nearly three weeks' standing.

In several cases the ordinary plans for reduction had been tried and failed, before or after admission; and one case, I remember distinctly, of subcoracoid dislocation of the shoulder-joint, in which the ordinary plans of "the heel in the axilla," etc., had been tried for nearly an hour, and without success, and which I reduced without any difficulty by manipulation. Another case, I can recall, of dislocation of the head of the femur on the dorsum ilii, in which the reduction was effected without any difficulty whatever, and with very little pain. In no instance have I found it necessary to use anæsthetics. The plan too is so simple that, after a little practice, the operator will find that he has little or no trouble in reducing any recent simple dislocation.

In order to make it as plain as possible, I shall now describe the mode of procedure, and take a case of dislocation of the right shoulder-joint as an example.

I placed myself on the same level as the patient, and on his right side, but in front of the limb, raised to a right angle, or higher or lower as I found most convenient, the elbow also being bent to nearly a right angle, and allowing the latter to rest on my left shoulder or upper arm, and using my left hand to steady the upper part of the arm, or control the scapula, if necessary, I inserted the tips of the fingers of my right hand into the axilla beneath, and to the inner and anterior or posterior part—according to the nature of the dislocation—of the head of the humerus, and, by gradually increasing pressure, lifted it quietly into the glenoid cavity, where I generally found it would go without much difficulty. If the bone got jammed at all or the patient were very muscular, a slight amount of traction, with or without a little rotary motion, or manipulation of the scapula with the left hand, I found would almost entirely overcome the difficulty; and the pressure on the head of the bone being continued, it would slip into its place. On the left side the treatment was just the same, the opposite hands, of course, being brought into use.

Dislocations of the elbow-joint I found most easy to reduce. I merely bent the patient's forearm on the arm to a right angle, or angle of 120°, and allowed his forearm to rest on my right forearm—in the case of a right dislocation, or on my left forearm—in the case of a left dislocation; or I supported the forearm with the inner fingers of both hands; and with the other fingers applied on the highest point of the bone dislocated upwards and backwards, and the thumbs on the lowest point of the humerus dislocated downwards and forwards, pressed the bones into their proper places. This dislocation can often be reduced by one hand only.

Although in luxation of the forearm backwards the coronoid process of the ulna is frequently broken off, it does not give any trouble. Other joints are treated on the same principle.

In the one case of dislocation of the hip, to which I referred, the man was brought in with the characteristic deformity of luxation of the head of the femur on the dorsum ilii, and was at once placed in an arm chair in a semi-recumbent position. I requested an assistant to raise the foot off the floor to a height of about twelve inches, and to rotate gently, whilst I placed my fingers beneath the head of the bone, and lifted it towards the acetabulum, into which it slipped without any difficulty.

Having as briefly as possible described my mode of procedure, I shall now conclude by stating what I consider the advantages of this plan.

1. It is simpler and more easy of application than the ordinary plans, and can be adopted anywhere and at once, as no surgical appliances are required.

2. As a consequence of the latter, no injury need be done to the limb or the parts around the joint, as the dragging of pulleys, bandages, heel in axilla, etc., are liable to do, owing to the fact that the fingers are the principal means employed for employing force, and there the sense of touch serves us.

3. It is attended with very little pain to the patient, and anæsthetics are seldom or never required.

P.S.—Since writing the above, two short papers have appeared in the British Medical Journal on the subject of reduction of dislocations by manipulation; but neither of them appear to me to have the advantages of the plan above described.—*British Medical Journal, Jan.* 7, 1882, *p.* 10.

38.—THE "DOUBLE SPLINT" IN AFFECTIONS OF THE HIP.

By A. Ernest Maylard, B.S., Lecturer and Demonstrator of Anatomy, Western Medical School, Glasgow.

To Mr. Bryant, perhaps more than to any one else, belongs the credit of introducing this splint in its present form, and explaining its special advantages; but it is hardly just to couple his name with it, thereby excluding others who constructed what he has slightly, though very materially, modified. If it should have a special name, it would be more just to call it Bryant's Modification of Campbell de Morgan's Splint—a long name—perhaps not unsuitable for so large a splint and its duplex character; but slightly, I think, too lengthy for common parlance, so I have preferred to call it simply the "*Double Splint.*" The affections of the hip in which the splint is especially applicable are cases of morbus coxarius, and fractures of the neck of the femur, whether intra- or extra-capsular.

To counteract the muscular action and to prevent contact of the inflamed articular surfaces extension is now almost universally adopted; but to prevent movement and to procure a proper position for the limb different methods are in use, and none that I have seen effectually secure either. Three kinds of apparatus are principally in vogue—one where simple extension is produced by a weight attached to a cord, passing from the limb over a pulley fixed at the foot of the bed, counter extension being produced by either raising the bed, as in Gordon Buck's method, or by passing a band beneath the axillæ, and fastening it to the bed above the head of the patient; another kind where, with a similar method of extension, a long outside splint is added; and a third, where the extension is produced from the side of the splint itself, counter extension being obtained by a perineal band and the splint fixed above by an abdominal or thoracic belt. The first, that of simple extension, is perhaps the worst, for not only is the patient frequently seen propped up upon his elbows, thereby flexing the thigh, but also wriggling about, sometimes on one side, sometimes on the other. It is needless to add, also, that in every movement of the patient for cleaning or for dressing purposes, when such are needed, the joint again suffers from want of rest; and, still further, there is the frequent absence of extension from either the weight resting upon the floor—the patient having managed to slip down in bed—or the cord has got dislodged from the wheel, and become jammed between it and the wheel support—a very frequent occurrence. Then, again, in removing the patient from the bed the extension has to be taken off. Another objection to this method is the one-sided "pull" which it produces, causing an abducted position of the limb, a position which is neither a good one for ankylosis nor, I venture to think, for rapid repair. For if it be true, as pathologists tell us, and anatomy also suggests, that the disease most frequently commences either in the capsule or lig. teres, this abducted position of the limb tends, on the one hand, to put into tension the inner part of the capsular lig.—that portion which Henle describes as the pubo-femoral; and, on the other, to jamb together more closely the swollen and inflamed lig. teres by approximating its femoral and cotyloid attachment. So that any movement in this position is likely to prove more deleterious than if the capsule be in a perfect state of relaxation, and the two attachments of the lig. teres slightly separated—conditions which exist when the limb is in a straight line with the trunk.

With regard to the second form of apparatus, that of the addition of a long outside splint to the above method of extension, it will be readily seen that many of the objections raised in connection with that will be also applicable here. There still remains the difficulty of moving or dressing the patient. Nor is the abducted position of the limb remedied. It possesses a slight advantage over the above, the thoracic or abdominal band preventing, though very ineffectually, the patient raising the upper part of the trunk, and the outside splint checking slightly the external rotation.

The third method of treatment where side splint and extension are combined in one apparatus is decidedly the best of the three, but still not destitute of many of the objections which exist in the other two. There are many forms of this kind of apparatus, as Bower's, Sayre's, Barwell's, M'Leod's, and others, all possessing one common source of objection—a perineal band. This is for counter extension, and, so far answers its purpose well, the fixed point and that which bears the main pull, being the tuber ischii, but considerable pressure is produced just over the head of the bone anteriorly, and just where in the earlier stage of the disease, when there is effusion, the joint is so acutely sensitive. Still further, the band becomes objectionable when at a later stage of the disease there may be discharging sinuses on the front or inner side of the thigh. I am aware that there are many cases in which the perineal band answers well, not only as serving for counter extension but also in keeping the pelvis applied to the splint, from which otherwise it nearly always sinks; but there still remain lacking those conditions needful for the perfect immobility of the joint, so that repair is both less rapid and less perfect. Abduction, though not produced in the same way as in the above two methods, is still not prevented; for the pressure of the splint upon the trochanter and crest of the ilium, induced by the way in which it is fastened above with an abnormal or thoracic band, causes it to assume that position by simply making a fulcrum of the projecting prominences of bone. This pressure, too, upon the trochanter, which no simple indentation of the pad entirely prevents, is an objection. The presence of a bracket in the side splint removes both these objections, and where with the bracket is coupled a foot piece and a cross piece the best kind of unilateral apparatus is obtained. The great advantage in this class of splint is undoubtedly the persistent extension which it permits. On the respective merits of the different methods of extension in each kind of splint I shall not enter.

To sum up concisely the criticisms I have ventured to make upon the different kinds of apparatus at present in use, and to be thereby better able to point out how they are dealt with in the double splint, I would enumerate the following:—

1st. The patient is not prevented from moving.

2nd. Any movement of the patient for dressing or nursing purposes, almost certainly involves movement of the joint, and not unfrequently causes pain.

3rd. There is a want of parallelism in the lower limbs, the affected side being caused to assume an abducted position with outward rotation.

4th. Extension is not always persistent, and when it is so, has to be produced by unavoidable pressure upon the joint, which may prove painful to the patient, and from its position inconvenient in cases of discharging sinuses.

5th. Pressure upon the trochanter by the unbracketed side splint causes abduction of the limb, and the head of the bone being driven into its socket, the inflamed surfaces are brought into contact, and movement which is permitted proves more deleterious.

6th. The abdominal or thoracic band employed to fix the splint above, when tight, tends, in the absence of a bracket, to lever out the limb, and when loose, as it frequently gets, permits the patient to flex the trunk upon the thigh.

When once the little one is tightly fixed in, there is a very poor chance of his getting loose. Indeed it is absolutely impossible for him to produce any movement save that of the arm, head, neck, and a slight tilting of the pelvis, which is possible in the dorsal decubitus. So that not only is the joint itself practically immovable, but any jar or shock from the movement of other parts prevented.

Then, as to the possibility of moving the patient for dressing or cleaning purposes without causing movement of the joint or pain to the patient. This is admirably effected by the splint; and had it no other claim for recognition, I think this feature alone would prove sufficient to ensure it. The patient can be easily lifted from one bed to another, or from one room to another; and as I have frequently done, carried patients into the open air, thus coupling with the local measures a very useful and essential item of constitutional treatment in many of this class of patients. With one hand beneath the buttock and one beneath the back, movement is easily effected. Again, the patient can be readily turned on to his side, thus giving convenient access to bed sores and to wounds, whether the result of the disease or the sequence of operation. All this can be easily effected without the least pain to the patient, and with the happy assurance that the natural process of repair is being in no way impeded or interfered with. The freedom from pain is frequently well marked. The little patients, when entering the hospital, cry at the least attempt at movement of the limb, but after a day or two in the splint, the acute sensitiveness of the joint subsides; and though there is at first a slight objection to so formidable a companion, they soon get used to it, and gain an affection for it from the security which they feel in its wooden embrace.

On the importance of parallelism of the limbs upon which Mr. Bryant lays such great stress, and indeed upon which he argues the splint has its greatest advantage, I am not inclined, with all due deference to his opinion, to wholly coincide. For, judging from results, it cannot be said that that position in which the limb becomes ankylosed under the one-sided treatment—if I may so express it—is so very unsatisfactory. Supposing the joint unite, as it usually does in an abducted position, the pelvis always accommodates itself, and not necessarily at the expense of any spinal curvature, but by the amount of motion which becomes developed at the sacro-iliac synchondrosis. Still, so far as outward appearance goes, the somewhat ungainly gait produced by the excessive rolling of the pelvis in ankylosis in the abducted position is materially lessened by union in the parallel. You will see by looking at the splint how well the abduction and external rotation of the limb is prevented. The extension, which can easily be applied to either side, is fixed to a sliding foot-piece, and to this the foot is firmly fastened, so that any tendency to outward rotation is checked. Counter extension is produced by downward pressure of the opposite foot upon the corresponding fixed foot-piece, the whole of the limb being bandaged to the splint, the knee cannot flex; and from the close application of the splint to the sides of the trunk above, it is impossible to produce any lateral deviation by the one-sided pull. Since, then, both abduction and rotation are prevented, there is an absence of all ligamentous tension; and since also counter extension is obtained at a point distant from the affected part, the joint is free from any pressure similar to that produced by the perineal band.

The presence of a bracket, seen on either side of the splint, removes all possibility of pressure over the joint, and so therefore of those particular parts of the articular surfaces and ligaments in which the disease most commonly commences. They permit the splint also to be easily applied and firmly fixed to the thorax above. The brackets again are particularly useful in allowing freedom of space for dressing in cases where there may be several discharging sinuses, or where excision of the joint has been performed. I think this will suggest one paramount advantage of the splint, the possibility of treating wounds of the joint without in any way disturbing its quietude and natural process of repair. The thoracic band, which is prevented from getting loose by a buckle fastening, passes through a small bracket on each side of the splint, and is thus always kept in position. It serves the double purpose of connecting together the upper part of the side of the splint, and prevents the patient from flexing the upper part of the trunk—principally, however, effecting the latter; the main connection above being produced by a metal cross-bar, which, like the rod below, allows, by means of screws, a lateral separation of the two halves of the splint, so that length is the only consideration in the fitting of it. It will be seen that the tightening either of the thoracic belt or the upper cross-bar can have no effect whatever in levering out the limb as in the one-sided apparatus, for the presence of the bracket removes all possibility of pressure upon the projecting prominence of bone which acted as a fulcrum in the above methods.

To sum up the advantages of the splint:—

The assurance of freedom of pain to the patient.

The convenience obtained for dressing and nursing purposes.

The perfect position of the limb.

The increased rapidity of repair from the impossible mobility of the joint, and the local and constitutional measures thereby permitted.—*Glasgow Medical Journal, March* 1882, *p.* 161.

39.—A NEW MATERIAL FOR COMMON FORMS OF SPLINTS.

By Walter Pye, Esq., Dublin.

For the past six or eight months I have used, as occasion served, a material for splinting the upper and lower extremities, which has proved very satisfactory (especially in children), which is very cheap, and which, as far as I know, has not been previously employed. The material in question is known as "corrugated paper;" is made by George Smyth, 47, Arlington Street; and is merely an application of the principle of giving strength, by fluting, to a material not inherently stiff, as one sees is effected in a porcupine's quill, or the feathers of a bird, or in the corrugated iron chapel. It consists of two pieces of very stout brown paper—one flat, and the other moulded into ridges and furrows. I may mention that the commercial use to which this paper is put is, I believe, principally that of either packing wine bottles in cases, or else of enabling single bottles to be wrapped up into a parcel, which shall have no external suggestion of the nature of its contents about it.

From the purely surgical point of view, however, I have found this corrugated paper extremely useful; it can be used in all those cases in which the somewhat expensive "kettle-holder" splinting may be employed, and makes about the best angular splint for the elbow I have ever seen. The tunnels of the fluted portion keep the limb cool by the circulation of air through them; and, moreover, if extra strength be needed, they will serve for the passage of wires of various thickness. It is so cheap, that for a shilling, material for about a dozen arm splints and the same number for the leg may be procured; it is very light, and may be cut with an ordinary pair of scissors.

It is unnecessary to point out that the qualities of stiffness in one direction, combined with flexibility in the other, will enable any surgeon to adapt the splinting material to a variety of cases requiring temporary or permanent support. It is not fitted for cases where there is profuse discharge, or where very damp dressings are brought into contact with it; for it readily becomes sodden, and then furnishes no support.

I have thought it worth while to describe this material, principally because I, as well as others to whom I have recommended it, have found it very useful to have a flexible splint—the cost of which is not worth considering (so that, if one do not fit, it may be thrown away and another applied); but also, because it seems to me to be a rather pretty example of the application of a well-known mechanical principle, the credit of which belongs to the maker of the goods, whose name I have given above.—*British Medical Journal, March* 18, 1882, *p*. 390.

40.—NEW SPLINTS FOR TREATING BOW-LEGS.

By G. R. Gilruth, Esq., Edinburgh.

V. A., a bow-legged child, aged three years, came under my care in January; she walked slowly, and the left ankle overlapped the boot so much that it was constantly coming off. For the treatment of this case I asked Mr. Gardner, cutler, to construct an apparatus, which consists of a rigid steel rod, fixed above to a thigh-piece made of thin copper shaped to the form of the thigh, and attached below to a foot-piece having a box on it to permit the rod to slide in and allow for any increase in the length of the tibia as it becomes straight. Sliding on this rod, so that pressure can be applied at any point, is a tourniquet screw, having a short leather splint attached to it by means of straps and buckles. This leather splint is placed over the curved portion of the tibia, and, when the screw is tightened, can be made to act very powerfully on it. The rod forms the inner splint and also the fulcrum for the screw, and being slightly convex—the concavity being towards the leg to which it is applied—it gives a better leverage, and prevents the skin from being pressed on.

The splints are applied in the following manner:—The foot and thigh-pieces are attached to the limb by broad strips of adhesive plaster; then a plaster of Paris bandage is applied over the foot-piece, and twisted as it is passed over the box, so as to allow the rod to slide freely. The plaster of Paris is next applied over the thigh-piece. The leather splint, well padded, is then placed over the curved portion of the tibia and secured to the screw by the buckles. The buckles should lie on the leather so as not to press on the skin. Pressure might now be made, but it seems better to wait until the plaster of Paris becomes perfectly hard.

On the 14th February the apparatus was applied to both legs by myself and Mr. Gardner.

On the 17th February, the plaster of Paris having slipped down on the left knee, the apparatus was removed and some improvements made upon it, and on taking the measurements again it was found that there was an improvement of an inch and a half. For instead of there being a distance of six inches between the anterior borders of the tibiæ—as was the case before the splints were applied—there was now a distance of only four and a half inches.

6th March. The screws were tightened every day at first, and then every second day. Cotton-wool was applied freely wherever there was the slightest pressure. Now the limbs are almost perfectly straight, and the screws might be removed, but the other parts of the apparatus will be kept on for some weeks. The child suffered little or no pain. It may be added that the handle of the screw is cut and turned into a key, so that the surgeon or the nurse can regulate the amount of pressure at pleasure.—*Edinburgh Med. Journ., April* 1882, *p.* 959.

41.—A NOVEL MODE OF APPLYING PLASTER BANDAGES.

By W. Newman, M.D.Lond., F.R.C.S., Surgeon to the Stamford Infirmary.

The material employed is a coarse-meshed muslin (tarlatane in France, crinoline or coarse book-muslin in our drapers' shops at home); costing about threepence per yard. A piece of this of sufficient length is taken and folded into a long bandage, having from sixteen to twenty-four thicknesses of the material, according to the requirement of the special case, and varying in width from two to three fingers' breadth. It is desirable, the folds being so numerous, that a loose thread tacking should be run along the free edges, to preserve the shape of the bandage and provide against unfolding.

Before proceeding further, the bandage should be soaked for a few minutes in water, so as to remove the stiffening or glaze which is liberally introduced in the process of manufacture of the material.

The plaster-of-Paris should be mixed with water (an equal bulk of each seems to be the most satisfactory proportion), in a large, rather shallow, bowl. As the mixing proceeds, the finely powdered plaster should be dredged on the surface of the water until it is exhausted. A mixture of creamy consistence will be the result.

Into this mixture the bandage, prepared as above, must be placed, and quietly moulded or kneaded, so that every part of it may be thoroughly permeated with the compound. It may be very loosely rolled up from each end towards the centre, for convenience of using; but there must be no attempt at compression or squeezing of the rolls as this is done. The desideratum is to have a bandage of very definite thickness, thoroughly and equally saturated with liquid plaster.

It will render the further description more easy, if the reader will think of the bandage thus prepared as affording, at the pleasure of the surgeon, either a narrow but thick band, or, when opened out by removal of the basting thread, a flattened, much wider, or expanded portion.

The narrower portion of the bandage permits application in simple ring or in figure-of-8 fashion around the terminal part of the fractured limb; and the wider expanded portion, carried upwards on the sides of the limb, furnishes a perfectly firm and well-moulded support to the injured part. A layer of wadding may be placed round the limb, so as to secure any projecting point from undue pressure, before the bandage is applied; or most commonly it will be found sufficient to rub the limb over with a fairly thick coating of oil or simple ointment. The plaster bandage will then not adhere to the skin.

I will speak first of the mode of application in simple fractures of the forearm. The arm, as is usual, must be placed in position between pronation and supination, and extension must be kept up so as to maintain the fractured ends of the bones in their proper relation one to the other. The middle of the bandage is applied to the front of, and then round, the wrist-joint. A ring is formed in this way, embracing firmly the lower end of the radius. One end of the bandage is now carried from behind between the thumb and finger, and downwards in its expanded portion over the anterior face of the forearm; while the other half of the bandage is taken directly from the ring over the wrist upwards over the posterior surface of the forearm.

Two long but narrow and rather strong bits of wood are now applied, one on each face of the forearm, with tolerable firmness; and a few turns of tarlatan bandage, *without plaster*, must be applied over these, to keep the whole framework in place until the drying and hardening of the plaster be fully completed. The advantage from these temporary wooden splints is, that the middle portion in length of each expanded portion is thus bent inwards, so as to correspond to the normal interval between the radius and ulna.

If there be simple fracture through the carpal end of the radius, the encircling ring is placed round the hand over the metacarpo-phalangeal joints; and the moulding of the wider portions of the bandage is commenced almost at once, so as to give support to the point of injury.

When the plaster is perfectly firm, the outer encircling bandage and the wooden splints are removed. A simple tie, elastic or other, will then suffice, if placed round the arm not far from the elbow, to keep the plaster framework in close apposition to the limb.

If simple fracture of the fibula, or of the tibia and fibula, transverse and without tendency to displacement, be in question, the bandage, prepared as above, is at once put on. The middle and narrow portion of the bandage is placed to correspond with the lower third of the leg posteriorly; the ends are brought round in front, and a complete close-fitting ring is so made; then they are brought down, each one over the corresponding malleolus, and crossed beneath the foot; finishing, finally, by the close application of the expanded portions, one on each side of the limb, nearly or quite up to the head of the tibia.

The best rule for the width of these expanded portions seems to be that, in the midline, both anteriorly and posteriorly, a vacant space of two fingers' breadth should be left between the corresponding edges of the plaster bandage.

Should the fracture of the leg be oblique, with more or less of riding and displacement, an ingenious adaptation of the plaster bandage is employed. To the leg itself a plaster bandage, as above named, is applied, with this variation only, that, before the crossing of the narrow portion is made beneath the foot, a roll of bandage is placed in close contact with the sole, and over this the crossing is managed. When the plaster has set, the bandage is taken away, and a projecting yet quite firm stirrup is left beyond the foot. This plaster bandage, so-called "extenseur," is covered with a layer of oil or simple ointment, and then an outside framework is arranged.

A long bandage is taken, soaked in plaster as directed. Between the layers a short piece of wood is slipped, to make an outer stirrup about three inches below that attached to the leg. The bandage is carried up outside the leg on each side to a point above the knee, and each side-prolongation is stiffened by the insertion of a piece of wood of the required length. The ends of the bandage are wound round the lower third of the thigh, so forming a complete ring, and enclosing the upper ends of the lateral wooden supports. This appliance is known as the "contre-extenseur."

The coating of oil or grease prevents any adhesion of the two bandages at the points where they are in contact. To complete the fixing, when the plaster shall have thoroughly set, the two stirrups, superior or foot attachment, and the inferior or outer framework, are tied together by a band of strong elastic. The extension so obtained will maintain the fractured ends of tibia and fibula in their proper place.

The especial advantages which may fairly be claimed for this mode of using plaster-of-Paris may, I think, thus be summed up. There is no covering up of the fracture or injured joint. The support, with proper care in the application, is abundantly firm from the first moment; secondary displacement, even with a restless patient, is impossible. The materials are inexpensive, and not difficult of use. The need for complicated apparatus in the treatment of the majority of fractures is done away with; and the comfort and convenience, especially to those of us who practise in the country, will, I believe, be found to be very great.—*Brit. Med. Journal, Dec.* 3, 1881, *p.* 893.

42.—CONTRACTION OF PALMAR AND PLANTAR FASCIÆ.

By A. S. Myrtle, M.D., Harrogate.

[Dr. Myrtle, of Harrogate, gives a very interesting account of his own case of this description. He says:]

The affection is neither hereditary with me, nor do any members of my family show any disposition to it. The little finger in my left hand is the only one deformed, and was traumatic from the beginning. I received a stab with a knife, after which the fascia began to thicken; and in a year, when I was seventeen, it reached its maximum, and remains like a butcher's hook.

The little finger in my right hand began to contract about fourteen years ago; eighteen months after that the ring finger commenced, and about four years ago the middle. Before the last began, the tip of my little finger was firmly imbedded in the palm of my hand, and the ring finger rested on its nail; both fingers were not only useless, but a constant source of annoyance, and I often wished they were off. Since Mr. Adams operated twenty-two months ago, my fingers are as useful as before the contraction. The tip of the little finger, when extended, is about three inches and a half from the palm; that of the ring finger four inches. The operation, as I formerly remarked, was not completed on account of rupture of the integument, as well as on account of some serious symptoms arising from the long continued inhalation of ether. I have met with scores of cases among men, but not one pure form among women. Occasionally I have seen a woman with contracted fingers, but these always showed rheumatic and gouty conditions of joints and tendons.

Hyperplasia, that is, the mere increase of substance, by addition to a structure already existing of matter identical to itself, is not inflammation; and this is what takes place in Dupuytren's contracted fasciæ. This is certainly not like any gouty or rheumatic action; it is neurotic, but every movement in the living organism is neurotic and, *ergo*, we need not classify this one under that head specially. I discard the theory that occupation has anything to do with the proximate cause, although I grant it may now and again prove the exciting one. If occupation could produce Dupuytren's contraction, most certainly we should find it very common among cavalry soldiers, whose left hands are constantly subjected to interrupted pressure from the reins, and whose right hands are equally tried by the sword-hilt. Army cavalry surgeons can surely enlighten us on this point. I have ridden and driven for forty years, and it is my right hand that has become contracted; the fingers in my left, barring the hooked one, are as straight and as supple as ever. I offer no apology for asking space for this note, as I know that the whole question is little understood by the profession; and many men are suffering a great deal of inconvenience on account of the indifference which this surgical defect has met with at the hands of even the greatest surgeons of the day.—*British Medical Journal, Jan.* 14, 1882, *p.* 48.

43.—NOTES ON THE SURGERY OF BURSÆ.

By W. MITCHELL BANKS, F.R.C.S., Surgeon to the Liverpool Royal Infirmary, &c.

The patellar bursa is subject to every form of enlargement, from slight distension with serous fluid up to the formation of a solid tumour. Pressure from kneeling being the almost invariable cause of this, domestic servants are in consequence its usual victims.

If the bursa remains in an irritated condition for any length of time the sac becomes thickened, and some active treatment is necessitated. Probably the best plan is to tap the bursa with a small trocar, and apply a blister over it, keeping the leg at rest on a back splint. As soon as the blistered surface will bear it, the knee should be strapped so as to induce the walls of the sac to cohere. Should this proceeding fail, and the fluid reaccumulate, the bursa may be again tapped, and a teaspoonful of strong iodine tincture thrown in, just as one does with a hydrocele. Some surgeons have warmly advocated the introduction of thread or worsted setons, but I cannot say that this is a proceeding of which I am much enamoured. My objection to it is that you set up suppuration in the bursa and at the same time do not provide a sufficient drain, so that a good deal more inflammatory mischief may be aroused than is required for cure. The subjects of the complaint under consideration are almost always treated as out-patients at hospitals. A thread seton is put in on a Monday, let us say, and the patient is directed to return on a Wednesday. But by Wednesday the knee is very painful, and she thinks she will wait till Thursday before coming up. On Thursday the pain is so bad that she cannot walk at all, and on Friday she is brought to hospital in a cab, with the knee frightfully red, swollen, and tense. Then she is taken in, the suppurating sac is freely laid open, and she is a month in bed. Having seen this actually occur I am somewhat chary about using setons.

But if the tapping and injecting, the blistering and strapping all fail, what is to be done? My present practice is to *excise the bursa* at once, and the principal object of the present little paper is to urge the more frequent adoption of this proceeding. In performing an operation which is not necessary for the saving of life (an "operation of complaisance," as the old surgeons would have termed it), one has to balance against the annoyance produced by the complaint, the pain of the operation at the moment of doing it, and the subsequent risk caused by it. With regard to pain, that is a thing of the past, as far as the work of the knife is concerned. As for the subsequent risk, antiseptics have put such an operation as removal of the bursa patellæ almost on a level with the commoner surgical proceedings of paring one's corns and cutting one's nails,—operations, by the way, which have both been followed by fatal results, but which, in spite of that, are universally practised.

Excision of the bursa is of course no new thing. It has long been performed, and is doubtless recommended in all surgical text books, but as a rule the recommendation is not pressing. On the contrary, the praise is decidedly faint. Holding the opinion which I have just expressed, I should naturally like to see excision advocated, not in the light of a dire extremity, but of a thing to be done on comparatively slight provocation. Hitherto it has practically been reserved for solid bursæ of considerable size, but I venture to think that it may be employed with advantage before the condition of solid tumour is arrived at. All bursal swellings are primarily cystic. They become solid, as I have often demonstrated in the theatre after an excision, in two ways, first by repeated deposits of lymph on the interior of the sac, and secondly by threads of lymph stretching across from wall to wall, which form tuberculæ and cut them up into cells, which in turn become filled with solid matter. My idea is to have them out before they have become actually solid—when they are, so to speak, semi-solid. For, after all, when they have reached this stage, tapping and injecting are thrown away upon them, and the only thing that will obliterate them is some such process as suppuration induced by a seton. Now, if the excision of a thick-walled or semi-solid bursa can be effected painlessly and safely, the next point to find out is whether it can compete with the seton in point of time. I think it can, and have come to the conclusion that a certain and permanent cure by removal does not take longer than that by prolonged suppuration, and is in all other respects much to be preferred.

In the method of operating I have ventured to make an innovation upon the ordinary practice of removal by a median incision. I have for some years made two incisions, one on each side of the joint midway between the edge of the patella and the femoral condyle. There seem to be two advantages which this method possesses. In place of there being after the operation a median cicatrix, which is painful to kneel upon, the skin in front of the patella is as sound as ever. This is no small advantage, seeing that the persons who get these bursal tumours generally make their living by kneeling, and have to take to kneeling again, even after they are removed. Indeed I was first induced to try the double lateral incision by the case of a coloured man, the cook and steward of a ship, who, in the performance of his multifarious duties, was necessarily a good deal on his knees, and so grew an immense bursal tumour. This was removed by the usual median incision. A prolonged suppuration ensued, the lips of the wound gaped, and a thick cicatrix resulted. He came to see me some months afterwards, and, although very grateful for the removal of his lump, complained not a little of the pain and inconvenience which the cicatrix occasioned him. In my next case the double lateral incision was employed with such satisfactory results that I have continued to employ it ever since.

The second advantage given by this method of removal is the excellent drainage obtained by it. This can never be got by the median incision, inasmuch as any effused liquid at once sinks down into the dependant parts of the pouch and pockets there. If suppuration occurs, the greatest trouble is sometimes experienced in getting quit of the pus. Morning and evening the cavity is washed out, and morning and evening it is found full again, until after considerable delay incisions have to be made at the sides, which might as well have been made at the beginning. So much have I been struck with the great advantage of drainage by lateral incisions that *I have applied the principle to all suppurating patellar bursæ.* In such cases a median incision (more especially if it be, what it too frequently is, a mere prick with a lancet) is most inefficient, and causes a great deal of unnecessary delay and trouble in the cure. Having ascertained with certainty the presence of pus, the patient should be anæsthetized and the suppurating sac cut down upon at as low a level as possible on one side. The finger should be inserted into the abscess cavity, and pushed over the patella to the most dependant point on the other side. The point of the finger should then be cut down upon. The incisions being kept open by a couple of large drainage tubes, the process of coalescence of the walls of the abscess is wonderfully accelerated.

While removing a bursa by the double incision, the great point is to keep steadily cutting upon the tumour itself. In front of the patella no possible mischief can be done, but just at its edge the capsule of the knee-joint is very thin indeed, and here there is a strong tendency to get off the tumour and go cutting downwards through the aponeurotic capsule. Although quite aware of this risk, I once inadvertently made a small hole into the joint. Fortunately I was operating under the spray, and by taking care that no blood got in, no harm resulted.—*Liverpool Med.-Chir. Journal, Jan.* 1882, *p.* 37.

44.—REMARKS ON TWO CASES OF MEDULLO-ARTHRITIS.

By J. Greig Smith, M.A., Surgeon to the Bristol Royal Infirmary.

We have in our medical vocabulary no single term to express that form of chronic joint disease which commences as an inflammation in the cancellated tissue of bone. "Osteoarthritis" would perhaps have been the most suitable name, but this has unfortunately been already made use of as a synonym for the rheumatic form of chronic joint disease. I say "unfortunately," because whatever else chronic osteoarthritis, usually so called, may be, it certainly is not primarily or essentially, as its name would imply, an inflammation of the bones in the neighbourhood of a joint. Among our best authorities—notably, Macnamara, Barwell, and Bryant—there is no want of outspokenness as to the radical differences between the synovitic and the ostitic forms of strumous joint disease. It may not therefore be premature to attempt to give them separate names. Partly because the term "osteoarthritis" is not available, but chiefly because the inflammation is one of marrow rather than of bone, I would call the disease starting in the cancellated ends of the long bones "medullo-arthritis." The other disease might then be named "synovio-arthritis." I believe that such a nomenclature, by calling prominent attention to their undoubted differences in origin and progress, would be beneficial as regards both science and practice.

I wish to direct attention to the distinctions between the two forms of so-called white swelling of joints, and specially to illustrate the progress and treatment of one of the forms by the two cases now before you. These chronic joint inflammations have long been called strumous, and I believe with justice. One commences as an inflammation of the synovial membrane, the other as an inflammation of the pink marrow in the cancellated ends of the long bones; and both ultimately proceed to disintegration of all the structures entering into the articulation. The first might conveniently be called "synovio-arthritis," the second "medullo-arthritis." You will understand the clinical differences between the two diseases more readily if I shortly describe to you their pathology. And, first, as to "medullo-arthritis."

You are aware of the fact that the pink marrow in cancellous bone belongs to the lymph-glandular class of organs, and probably discharges most of the functions of ordinary lymphatic glands. You are also aware that in a certain habit of body called "strumous," the lymphatic tissues are liable to a peculiar form of inflammation. This form of inflammation is characterised by its chronicity, and by a tendency for its products to retrograde and undergo the form of degeneration called "caseation." The similarity as to structure and function between pink marrow and lymph-glandular tissue in health is still further borne out in disease. Wherever we find pink marrow, in the ends of the long bones, in the bodies of the vertebræ, in the bones of the hands and feet, there we frequently find it affected with a form of inflammatory disease similar to that found in the strumous diseases of lymphatic glands in connective tissue. The inflammatory products are of the same histological type; they show the same sluggishness, and have a like tendency to undergo the caseous metamorphosis. Thus far the likeness is complete, but hereafter there are important divergences.

A strumous lymphatic gland in the neck has room to swell, and, if it suppurates, its contents perforate the skin, and so are discharged from the system. But it is not so with the bone glands. They are bound down inside a bony shell, and the swelling, which is a sequence of the inflammatory process, results in compression and strangulation. But the inflammation proceeds, and the inflamed, perhaps suppurating, tissues force an outlet wherever they most easily can. In the bones of the hands and feet the compact shell is thinnest on the dorsal aspects, and there we find the opening. In the vertebræ the perforation fortunately takes place on the anterior aspect. But in the ends of the long bones the inflamed marrow usually takes the most dangerous course—through the articular cartilage into the joint cavity. Elsewhere the terminations are often serious enough; but here the results are peculiarly grave. When the marrow abscess—for so it may be called—bursts into a joint a form of suppurative synovitis is set up, which, if untreated, will infallibly lead to complete destruction of the articulation, perhaps even to loss of the patient's life. With the most skilful treatment the disease is always grave; indeed I should probably be correct in saying that it is our most prolific sourse of amputations of the thigh.

The other form of strumous arthritic disease is primarily and essentially one of the synovial membrane. What is the special pathological feature of the disease I cannot here stay to discuss. Suffice it to say that the synovial tissue is transformed into a pulpy gelatinous-looking material which is always œdematous, and sometimes, especially in the more advanced cases, infiltrated with collections of caseous or purulent matter. Histologically the structure is lymph-adenoid; and frequently we find it infiltrated with tubercles, or at least with globular aggregations which receive that name. I show you some specimens in which great numbers of the so-called tubercles can be seen. Note the beautifully regular concentric arrangement of the cells in these bodies. They are rounded in the centre, flattened and tailed at the periphery, suggesting the structure of a concentric globe in epithelioma. Very likely these ought to be classified as tubercles; individually I believe they are lymphatics which have become blocked by the proliferation of their endothelium. It is, I think, probable that lymphatic obstruction is an important element in the causation of the disease. The amount of œdema and the abundance of immature and degenerate lymph cells present in every case lend a high probability to such a view.

This lowly organised pulpy granulation-tissue is endowed with a peculiar tendency to infect, or absorb, or assimilate—call it what you will—every form of fibrous tissue which it meets. The tendinous structures in and around the joint always suffer, and the fungating tissue creeps on from the periphery over the articular cartilage, corroding and absorbing or transforming it in its course. By-and-by the synovitic material completely eats through the articular cartilage, and, so to speak, taps the marrow cavity. In the pink marrow it meets with a tissue most congenial for infection, and matters now proceed apace to the complete disintegration of the joint.

Such is a short *résumé* of the natural history of the two forms of disease. Let me now, by the help of these preparations, contrast them more in detail.

Here are three specimens of knee-joints affected with medullo-arthritis. Note in the first place the slight amount of synovial thickening. In each you see that the articular cartilage on the end of the femur is perforated at several points, and that around each perforation the cartilage is everted and thinned out from below. Here and there a piece of cartilage is elevated like a blister; there perforation by fungating marrow is about to take place. At one part a mass of granulation material protrudes from the end of the femur under the patella, and the opposing patellar cartilage is corroded on its surface. The same may be seen on the head of the tibia where a corroded area was in contact with another mass of protruding granulations.

In contrast with these, note the appearances in these joints affected with synovio-arthritis. There is enormous thickening of the synovial tissue. In all the joint cavity is lessened, in some almost completely obliterated, by the inward growth of the gelatinous pulpy tissue. By lifting up the diseased material from the cartilage you can see how it is corroded and eaten away under it. This fresh section shows the gradual growth inwards over the joint surface, and the progressive absorption of the articular cartilage. Everywhere the cartilage is corroded on the surface; nowhere is it perforated from below. Here, where the marrow cavity has been tapped, you can see nothing of the eversion of the edges of cartilage which you see in the other specimens. The crucial ligaments in one specimen are so completely infected and transformed that you can see a series of granulation tufts sent out from them to eat up the cartilage over the condyles. I peel these off, and the corroded cartilage is perfectly sound, and firmly adherent to the bone beneath.

You are now in a position more readily to understand the clinical distinctions between these two diseases. In the synovial disease there is little hyperæmia, and except in the advanced stage, little pain. Even when there is some abrasion of the cartilage, the flabby anæmic granulations, packed between the denuded surfaces, act as buffers and prevent pain. Grating inside a joint is supposed to mean "ulceration of cartilage," but, as has often been pointed out to you, there may be much abrasion without any grating when masses of synovial granulations serve to separate the articular surfaces. A joint may be far advanced in synovial disease, and still be used in movement or walking without causing much pain. Perhaps the most important diagnostic fact in synovial disease is that until in the last stages, when these pulpy granulations have broken down and suppurated, there is no "starting" in the joint as the patient drops off to sleep.

In medullo-arthritis, again, where we have a highly sensitive and vascular organ compressed inside a bony shell, there is tenderness during, and great dread of any sharp movement. Percussion around the joint causes pain. Long before the cartilage is perforated the patient is a cripple, and when the marrow has forced its way through, and a suppurative synovitis is added to the osteo-myelitis, there is always great pain, often extreme agony. The exposed cancellated bones rubbing on each other must be credited with this. Here the synovial thickening is not enough to prevent the raw surfaces from grating. It is in this disease by pre-eminence that we get nocturnal startings.

On inspection and palpation differences may be detected. A joint affected with synovio-arthritis has pale, smooth, sometimes glassy and lustrous skin, and large blue, but somewhat deeply-set, veins coursing over it. In medullo-arthritis the skin is not pale, but of an obscure dingy red; instead of being smooth, it is usually rather rough and mottled, and is frequently covered with long hairs from the patient keeping it at rest under his clothing on account of the pain.

When the synovial membrane becomes fully implicated we may, however, find a close resemblance to pure synovio-arthritis. The peculiar sensation of bogginess in a synovitic is not present, or is less marked in an ostitic joint. Brawniness in the earlier stages, at least, would better characterise the condition in the latter form. Generally we might say that stiffness and dread of movement were characteristics of the ostitic disease. In both cases, from softening of ligaments, there is undue lateral mobility.

Such are the leading clinical distinctions between the two diseases. Each case must be judged by itself, and this you have daily opportunities of doing in our wards. When abscesses have formed and sinuses exist, you may conclude that the two diseases co-exist; but you should still endeavour to find out whether marrow or synovial membrane was the original cause, and direct your treatment accordingly.

The two cases I now show you are intended to illustrate the treatment of medullo-arthritis. They are not exactly typical cases of the disease, being more acute and more grave than we usually meet with; but I hope they will be all the more impressive and instructive on that account.

Our first patient, a girl of fourteen, was admitted in a wretched condition some months ago. She was much emaciated, and suffered from continuous diarrhœa. She had been bedridden for some weeks with pain in her left knee, but beyond this fact we could get no trustworthy history. On examination, the joint cavity was full of fluid, and there co-existed a peri-articular abscess over the outer condyle. Her evening temperature was 103°. She complained of great pain in the joint. Next morning this abscess was opened, and as the pus flowed the joint also emptied itself. In our manipulations of the joint some air (antiseptic, of course) got in, and we could feel the bubbling and crackling inside. Next night the temperature was 98·8°, and the other symptoms were somewhat improved. For three weeks the knee was dressed on every third day; but matters did not progress satisfactorily, and towards the end of the third week her evening temperatures had again risen to 102°. The cause of this was found to be a huge abscess over the right buttock. This was opened, dressed antiseptically three times during three weeks, and at the end of that time was perfectly healed. In the meantime her temperature had remained nearly normal, and she was gaining flesh and strength. But her temperature suddenly went up again, with pain in the other knee-joint. Rest, cotton-wool, &c., had no influence, and there was no doubt that this knee also was suppurating. Albumen was at this time found in the urine.

It was now clear that we had to deal with a condition of pyæmia. We know that osteo-myelitis is a frequent cause of pyæmia, and we had every reason to believe that suppuration was going on inside the condyles of our patient's left femur. The indication was clear: to give a free exit to the suppurating marrow bound down inside the femur, and engaged in the meshes of its cancellous bone. The only other alternative was amputation.

Six ounces of flaky pus were removed from the right knee-joint, and two large drainage-tubes were inserted. On the left limb an opening was made by a gouge towards the outer and posterior aspect of the outer condyle, and the contents of both condyles were scraped out. Sufficient proof of the existence of disease was given in the fact that a Volkmann's spoon was strong enough to break down the cancellous bone inside. With the little finger in the opening it was possible to feel the articular cartilage. A large drainage-tube was inserted into the bottom of the cavity in the condyles.

From that time to the present, when you see the patient jump down from a chair and walk about without lameness, the course was one of uninterrupted improvement. The temperature became normal and continued so. Diarrhœa ceased, as did the albuminuria. In ten days, after two antiseptic dressings, the wounds in the right knee-joint were closed and skinned over. Antiseptic dressings were applied to the left knee-joint for six weeks, when they were changed for boracic lint. The cavity inside the condyles is now filled up, and there remains only a small superficial sore. I may add that we were careful to move both joints every time we dressed them.

Our other patient is also a girl of fourteen. She came from South Wales some months ago with suppuration inside the right knee-joint, starting from osteo-myelitis of the lower end of the femur. She had been six weeks ill in bed, but had been lame for some weeks previously. The whole of the right limb was swollen and in parts œdematous; there was considerable bony thickening of the lower half of the femur, and the joint cavity was enormously distended by fluid, which was on the point of bursting through the skin on the inner side of the patella. There existed in this case also a peri-articular abscess, produced apparently by bursting of the joint capsule. I need not say that there were much pain and starting with considerable fever. Here, then, was a case of osteo-myelitis of the end of the femur, causing an abscess, which had burst into the knee-joint. It differed from an ordinary medullo-arthritis, in extending for some distance up the femur, and from pure osteo-myelitis in being so chronic.

At first, as in the other case, we tried simple drainage of the joint. Matters were considerably improved thereby, but it was clear after three months' treatment that the real cause of the disease had not been removed. Accordingly, exactly the same operation was performed as in the previous case. The whole of the contents of the femoral condyles were removed through an opening made (in this case) on the inner side. After all the softened tissues had been removed, the finger introduced through the opening not only could feel the cap of the articular cartilage, but could make it pout at the sides of the ligamentum patellæ. A large drainage tube was inserted and retained for six weeks. At the end of that time it was removed; antiseptics were stopped and boracic lint dressing applied. There is still some slight discharge, but this is daily diminishing, and the patient will soon, I hope, be walking about. Here the result will not be so good as in the other case, on account of the adhesions which have formed. I did not dare to institute passive movement here, because the articular cartilage was left with so little support. And as the cartilage will give us some of the new bone destined to fill up the cavity which we made, we must be careful not to interfere with its nutrition more than we can help. Besides, I believe it had contracted adhesions to the tibial cartilage during the period the joint was being drained.

These were aggravated instances of medullo-arthritis; and I call your attention specially to these as the most powerful form of advocacy I can employ in support of what I believe to be the best treatment of all cases of medullo-arthritis when simple measures of rest, counter-irritation, &c., have failed. And I would venture to recommend it specially in the early stages of the disease. In the later stages, when the soft parts are disorganised and the bones carious, we are driven to mutilation of the limb—amputation or excision. "It will come to excision" is an expression very often applied to these forms of joint disease. It is an opprobrium to our art that we should so frequently have to stand by with folded hands while such cases doggedly go on to that extent which will justify the major operation.

I believe that if the pathological condition is recognised sufficiently early the progress of the disease might be nipped in the bud. If it is an area of inflamed marrow that is the root of the evil, and if we can reach and remove this, we ought to cure the disease. If there be any justification for the treatment of strumous glands in the neck by removal, there is much stronger justification for the removal of a similarly diseased structure inside bone. Inside a bony shell everything is against spontaneous cure. Compressed and perhaps strangulated by the outer shell the inflammatory products are driven to destruction; and, when decay has set in, the abundant bony trabeculæ entangle the degenerate products and delay their discharge through any opening that may have formed. The very chronicity of its course begets thoroughness of destruction. Long after the marrow is in a state of suppurative or caseous degeneration the bony trabeculæ remain to confine it, simply because, being now almost bereft of vitality, the decayed tissues cannot cause bony absorption or disintegration. Bony caries is always a tedious complaint.

Everything, therefore, points to a justification of the early treatment of such diseases. Spontaneous cure is so rare and the ulterior effects are so disastrous, that any operative procedure which has a sound pathological foundation, does not endanger life or limb, and promises to cure the disease, is sure to be welcomed by surgeons. The tapping of a marrow cavity and scooping out more or less of the inflamed marrow and bony trabeculæ is not a severe operation, and it seems to me to be a sound one. It is probably a question more of diagnosis than anything else. If you are satisfied that chronic medullitis exists inside a bone, and you find that ordinary measures are ineffectual, I think you ought to provide an exit for the inflammatory tissues, otherwise they will assuredly make an exit for themselves along a channel that will almost certainly cause destruction of the articulation, or even of the limb.

The early treatment of chronic joint disease is, at the present day, one of the most pressing questions in surgery. Recent discussions on this point have done little else beyond indicating our general therapeutic helplessness. An exception ought to be made in favour of Mr. Teale's plan of giving an exit to fluid which is distending a joint in the earlier stages of the synovial disease. By relieving tension inside an inflamed joint one continuing cause of the mischief is, for the time at least, removed. But the chief cause remains, for we find that such a simple measure is rarely sufficient to cure the disease. For the other form of disease, medullo-arthritis, I know of no early form of treatment except the universal one for all inflammations—rest.

And how unsatisfactory this is, the great number of joint excisions annually performed in our large hospitals is sufficient testimony.—*Lancet, Dec.* 24 *and* 31, 1881, *pp.* 1077, 1117.

ORGANS OF CIRCULATION.

45.—EXPERIENCE OF OX-AORTA LIGATURES.

By Richard Barwell, F.R.C.S., Senior Surgeon to the Charing Cross Hospital.

Two years ago, namely, on 10th March, 1879, I had the honour to read a paper, the object of which was threefold. First, To point out that certain cases of aortic aneurism might be benefited by deligation of the right carotid and subclavian arteries—an operation which had not previously been proposed or practised for that purpose. Secondly, To insist upon the advisability of leaving the inner and middle coats of arteries tied in continuity intact. Thirdly to introduce a form of ligature which in deligation would not cut those tunics. At that time the experience which I could place at the service of the Society was limited to the one example, which illustrated all those requirements. Even now, cases which vindicate all the three points I then advanced must in the nature of things be few.

Since the publication of my paper there have been two cases of deligation of the right carotid and subclavian for aneurism of the aortic arch with the ox-aorta ligature, making in all three such operations. The first one by myself, on the 14th Feb. 1879 (the subject of my former communication). The second by Dr. Lediard, at that time of the Cleveland Street Infirmary, operated 26th March, 1880. The third by Dr. Wyeth, of New York, operated Sept. 29th, 1880. My patient lived sixteen months in fair health and comfort, dying in May 1880. Dr. Lediard's patient survived rather more than nine months, dying on 22nd Dec. 1880. Dr. Wyeth's patient is alive. "She is up and able to move about without distress or difficulty. Has no dysphagia or dyspnœa, and no pain in the tumour, which still rapidly diminishes." (Report on Christmas Day, 1880.)

This is the list of the right subclavian and carotid vessels hitherto tied with ox-aorta ligature. On October 27th I tied the vessels on the left side of the neck of J. K——, æt. 37, suffering from a large thoracic aneurism springing from the transverse arch of the aorta, beyond the origin of the innominate. The man was, previously to operation, evidently approaching death. It was thought that deligation of those vessels might give some chance of prolonging life; it failed, however, to do so, but I do not think it hastened death, which occurred thirty hours after from pneumonia.

The parts of my first named case, J. S——, are on the table, as also, by the kind permission of Dr. Hopkins, of the Cleveland Street Infirmary, are the aorta, aneurism, &c., of Dr. Lediard's patient, while of my last-named patient I have brought only part of one vessel and the ligatures used.

I will now describe the condition of things found in my patient, J. S——, sixteen months and a half after operation. On the aorta are two aneurisms. A large one in front on the transverse aorta, and slightly involving the innominate, is filled with solid clot. Behind and springing from the ascending trunk is another aneurism, about the size of a walnut, exactly in the situation, where I, during the patient's life defined it from the peculiarly isolated pressure which bore on the right bronchus. The carotid of this man was the first vessel that I had tied with the aorta ligature, and it is clear that I tied it too loosely. The temporal pulse could be felt almost immediately after the operation; this I at the time attributed (but evidently by error) to freedom of collateral circulation. The ring of cellular tissue which the College of Surgeons' dissector has left on that vessel is probably the remains of the ligature. On the subclavian, at the place of constriction, a few faint transverse striæ could be seen—the vessel is collapsed—it has been opened by a lengthwise slit in order to show the clot which fills it for a certain distance on each side the constriction. The collateral circulation is well established.

The description of parts from the next case I take from a letter addressed to me by Dr. Lediard:—"The aneurism is filled up with very firm clot. Nothing could have been more satisfactory than the action of your ligature in this case. The vessels were entirely occluded, there being no trace of the bands seen but a firm clot above and below the seat of constriction, such as is usually met with in vessels ligatured in their continuity."

The other specimens on the table, those of J. K——, are:—1. The part of the subclavian artery which was tied; it is laid open to show the inner and middle coat entirely unhurt. 2. The ligature itself, which was divided in removing the parts. 3. The other ligature, kept with the knot entire in its tied condition; it has been removed by dividing the vessel. Evidently there is no tendency of the knot to slip, nor of the ligature to become rapidly softened.

Certain other vessels have been tied with this ligature, viz. the external iliac twice, by Mr. Holmes and Mr. Johnson Smith. Both those aneurisms progressed well, and the surgeons reported to me very favourably of the ligature. Also, femoral arteries have been tied by Mr. Bellamy twice, by myself once, and one brachial also by me. In all these cases the wound did well, nothing more having been heard or seen of the ligature; indeed, in the last four very rapid cure followed. In the brachial case the vessel was retied with ox-aorta after failure from rapid softening of catgut and secondary hemorrhage.

Thus I am able to report to this Society fourteen deligations, which being varied, are more than sufficient to prove the solubility of the ligature which I introduced two years ago, and also to show that it is not absorbed too soon. Hitherto no instance of tying either the innominate or the first part of the subclavian has presented itself. I would wish, however, to repeat that the deligation of such large vessels so near the heart can only be safely undertaken with a ligature, which leaves all the arterial tunics entire and unwounded, a result only to be obtained by using a flat or ribbon-like band.

The specimens which I have placed on the table, six in number, especially the last described, show how the ligatures have acted. Let me very specially call attention to the fact that ox-aorta requires no preparation; it is simply cut, while fresh, into strips and dried; therefore all these ligatures are in their constitution alike. What these have done all others may certainly be expected to do.

I do not for one moment suppose that this power of absorption by the living human tissues is peculiar to arterial tunics, we know that all the soft connective tissues, as tendon and cellular membrane, possess this faculty, provided they are perfectly fresh, that is to say, not decomposed.

The extensor or suspensory tendon of the kangaroo's tail, tendons from the whale, decalcified bone from quadrupeds and birds, have all been introduced into the human body and have become absorbed or incorporated with the tissues. The (fibrous) structure which intervenes between the mucous and muscular coat of the sheep's intestine, the substance of which catgut is made, would doubtless act constantly and always in the same way were it not that a certain, or rather an uncertain, amount of putrefaction is essential in the primary manufacture; it is this that renders a long and elaborate preparation necessary, and the ultimate result uncertain. To me the important points appear to be that the substance used should be absolutely free of all taint of decomposition, and that it should be cleansed from any accidental impurity by a short immersion in some antiseptic fluid. If these essentials be secured the choice of particular substance is immaterial; sufficient strength, persistency, ability to keep a safe knot, as also facility of acquisition, are the main considerations for an ordinary ligature. But for safe deligation of the larger arteries in continuity a flat form such as will not divide the tunics, is a great desideratum.—*Medico-Chirurgical Transactions,* 1881, *p.* 225.

46.—ON THE USE OF THE CATGUT LIGATURE.

By John Gay French, M.D., F.R.C.S. Eng., Late Civil Surgeon of Dacca, Bengal.

Mr. Richard Barwell's very interesting and instructive case, published in the Lancet of July 30th, will tend to raise doubts in the minds of surgeons as to the utility and real security of the catgut ligature when applied to an artery. I myself am perfectly satisfied with it, having used it exclusively in practice, and tested it in experiments on animals; and in the following observations I may, perhaps, be able to throw a little light on the cause of failure in some instances. Even if my conclusions be not accepted as correct, I suggest points for further experiment. The chief items in Mr. Barwell's case, to which I beg briefly to refer, are as follows:—A child, aged seven years, received an injury which caused division of the ulnar artery, and this, after suitable methods of treatment at each stage, resulted in an attack of hemorrhage, for which the brachial was tied. The operation was successfully performed with the "thickest carbolised catgut" available. For nearly seven days hemorrhage was entirely restrained, but then it came on suddenly and in considerable quantity from the primary accidental wound and from the operation. Mr. Barwell reopened the operation wound, and discovered the "knot of the catgut ligature lying upon the artery, but quite loose. Its noose, that part which ought to have encircled the vessel, had disappeared. Under this knot the artery, not at all constricted, was partially divided, its coats cut clean through, as with a knife, for about a third of its periphery. Out of this rift blood freely spouted, when command of the vessel above was relinquished." Mr. Barwell tied the artery above and below the wounded part with a ligature made from ox-aorta, and the result was very satisfactory.

With this case compare results in the following experiment: In December 1879, in Patna, I tied the femoral artery of a young dog, of about nine months old, with the thickest catgut ligature I could procure. Everything went on well until the 13th day, when profuse secondary hemorrhage set in. After death I examined the site of operation. Except a small hole, through which the blood had issued, the wound was nearly healed. On cutting down, I came on the knot of the ligature lying on the coats of the artery, which were roughened and in a semi-sloughy condition. The artery was quite pervious below the ligature, and on lifting up the knot, an irregular opening into the vessel was seen underneath and above it. Through this ulcer the hemorrhage had occurred, and no trace of the noose was found. I may here remark that in many other experiments on dogs I had no case of secondary hemorrhage, but then they were chiefly, if not altogether, performed with medium or small-sized threads.

Now, in these two cases the chief point of similarity is that in both the *thickest carbolised catgut made* was used, and herein lies, I think, the fault and cause of the secondary hemorrhage. After the ligature of the brachial artery in the child, hemorrhage was restrained for nearly seven days, showing that a complete stop had been effected on the circulation; a thrombus had formed, and probably the normal hæmostatic processes for occlusion had progressed favourably. Some time antecedent (? fifth day) to the date of hemorrhage, the noose of the ligature had dissolved and disappeared, leaving only the large knot, which, resisting the process of softening and disintegration, acted as a foreign body, and caused ulceration of the subjacent coats of the artery and secondary hemorrhage. In a similar manner the old silk or hempen ligature, when coming away and acting as a foreign body, caused in some cases ulceration or sloughing of the coats of the vessel and secondary hemorrhage. In 1864 I tied the femoral artery in Scarpa's triangle for traumatic (gunshot wound) aneurism in the centre of the thigh, using stout silk for the purpose. The result was successful, but before the wound had healed there was, upon two occasions, considerable secondary hemorrhage. In the case of the silk ligature the fault lies in the material, which must necessarily cause a destructive process for its elimination from the wound. In the case of the catgut, the fault is not in the ligature itself, but in the want of adaptation of the size of the cord to the vessel exposed, or to the want of antiseptic precautions in the treatment of the wound. In other words, a very thick catgut ligature to an ordinary-sized vessel will probably fail, whereas a small- or medium-sized thread will ensure a satisfactory result. To those who are in a position to carry out experiments I venture to suggest the advisability of determining whether, in many such cases, the secondary hemorrhage is not chiefly due to the employment of catgut of too large a size. Prof. Lister tells us that "for tying an arterial trunk in its continuity, catgut as thick, when dry, as ordinary purse silk will be found best. But for ordinary wounds, where, if one ligature happens to break, another can be easily applied, much finer kinds may be employed, and are convenient from their smaller bulk." To this I would add, that when an artery has been successfully tied with the finer kinds of catgut the result will be more satisfactory than if thick strands had been used.

In septic cases the influence of the tissues on carbolised catgut must be clearly borne in mind. This has been well pointed out and demonstrated by Prof. Lister himself, Drs. George Beatson and W. J. Fleming of Glasgow, and others. In my own cases I would use a fine or medium-sized thread, and fully carry out the antiseptic system in each. While I was civil surgeon of Patna in 1879, and of Dacca in 1880-81, in all our operations in the Temple Medical School Hospital, and in the Dacca Mitford Hospital, we used no other ligature for arteries or vessels than catgut, and always with satisfactory results.—*Lancet, Nov.* 19, 1881, *p.* 866.

47.—RAPID METHOD OF CURE OF EXTERNAL ANEURISM BY MEANS OF THE ELASTIC BANDAGE.

By A. Pearce Gould, M.S., F.R.C.S., Assistant Surgeon to Westminster Hospital, Surgeon to the North-West London Hospital, &c.

In 1864 a case of aneurism of the abdominal aorta came under the care of Dr. Wm. Murray, of Newcastle, who, finding that he was able to control the pulsation in the tumour by digital compression of the vessel above it, formed, and carried to a successful issue, the plan of completely stopping the flow of blood into the aneurism by instrumental pressure—pain being allayed by anæsthesia—and so introduced what he termed "the rapid method" of treating aneurism. In his description of this case and method, Dr. Murray particularly points out the nature of the curative process, and remarks that the "*principle* on which the rapid method rests is clearly the complete stagnation of a mass of blood in the aneurism until it coagulates. Whether this is to be done under chloroform or without it; whether by pressure both above and below, or only above the sac; and whether the pressure is to be made by the hand, by flexion, by a weight, or by a tourniquet, are questions which in no way touch the principle on which the treatment is based."

In the limited class of internal aneurisms where this rapid method is practicable, it has been tried in several instances. In 1865 Dr. E. D. Mapother attempted to carry it out in the treatment of external aneurisms, and recorded two successful cases in the British Medical Journal, 1867. But he lacked easily applied safe and efficient means of entirely stopping the circulation in the part, for in each of his cases the compressor caused a local slough; and it was not until ten years later that the introduction of Esmarch's elastic bandage supplied this want. In 1875 Staff-Surgeon Walter Reid, R.N., operated on a case of necrosis of the femur, and during the whole period of the operation, which was protracted to over an hour, the limb was kept quite anæmic by means of Esmarch's elastic tourniquet. Having a case of popliteal aneurism in the hospital at the time, it occurred to Dr. Reid that this same tourniquet would be a ready and safe means of keeping the sac full of stagnant blood for a time sufficient for its coagulation. Accordingly, next day (Sept. 11, 1875), at 10.20 a.m., he "bandaged the limb with the elastic roller, from the toes upwards to the junction of the middle with the lower third of the thigh. It was applied lightly over the aneurism, so as not to compress it. The elastic tubing was then wound round the limb over the highest turn of the bandage, which was now removed. The entire circulation below the tubing was found to be arrested. The limb had assumed a death-like pallor, and gradually lost temperature. The aneurism was of its usual size, and pulseless. After fifty minutes, the patient complaining of severe pain above the seat of constriction, a Carte's compressor was adjusted to the main trunk at the pelvic brim, and the elastic tubing removed." The aneurism was now found to be pulseless, and several small vessels round the knee-joint were felt beating strongly. Carte's compressor was applied lightly and intermittently, so as not to distress the patient, till the next evening, when all treatment was suspended, and the aneurism was cured. Dr. Reid, referring to the case, remarks:—"The explanation appears to be—the coagulation of the blood in the sac, consequent upon its complete stagnation. The aneurism itself probably remained full, but the entire circulation in the parts was arrested. . . . . My reason for compressing the main trunk for some time afterwards was that the force of the blood-current might have broken down and washed away the newly-formed clot before it had time to become tough and solid, and the sac to contract over it." From these quotations it is demonstrated that Dr. Reid clearly appreciated the principles involved in the practice he initiated, and those who have followed him have not been able to make any important additions to the details to be observed to insure success. This first case was so strikingly successful that the method was soon employed in other cases by other surgeons, but it is not until quite recently that continental surgeons have availed themselves of this latest development of the plan of treating aneurisms by compression. I have collected more than seventy cases of external aneurism treated by Esmarch's bandage, which have been recorded more or less fully.

Laminated fibrin is only separated from blood in motion; when once formed it is very stable, not prone to disintegration or to *organisation. Blood-clot is only formed when the blood is at absolute or partial rest; and when formed the clot is unstable, readily being organised, disintegrated, absorbed, or converted into a dry friable material.*

The means hitherto employed in the treatment of aneurism have chiefly aimed at the obliteration of the sac by a gradual deposit of laminated fibrin, Dr. Murray's "rapid treatment," manipulation of the sac and injection of coagulants, being the only exceptions. But the history of some cases of digital and instrumental compression makes it abundantly clear that the cure has really taken place through sudden clotting of the blood, for sometimes after treatment has been long continued and unavailing, the aneurism has very rapidly, and even suddenly, become solid and pulseless. With the means at the surgeon's disposal, Dr. Murray's "rapid treatment" has been of but very limited application; that its principle was the attainment of stasis of the blood in an aneurismal sac for a time sufficiently long to allow it to coagulate *en masse*, there can be no doubt.

Reid's treatment is usually spoken of as a variety of the "Compression Treatment;" but as a distinct appreciation of the special effects it produces both locally and generally—effects widely different from those of digital or instrumental compression, or even the ligature—is necessary to its successful application, it will be well to note these points of difference in detail.

If we take the common case of a popliteal aneurism, treated by digital compression of the femoral artery at the groin, we observe that the main artery is more or less perfectly obliterated at one point, that care is taken to have the compressing force limited to the artery, and not to act upon the companion vein if possible; the anastomotic circulation is not interfered with, and therefore the blood soon finds it way into the femoral artery below the finger, and flows through the aneurism in a gently trickling stream. This compression has to be continued for many hours, or at intervals for days or even weeks, and it usually succeeds as well (though not so quickly) when intermittent as when continuous. When successful, the aneurism is found to become gradually firmer with lessening pulsation, until at last it loses pulsation altogether, its thrill and bruit are no longer perceptible, and the tumour is solid; then the artery from which it springs becomes occluded, and those around the knee-joint are found to become enlarged and carry on the anastomatic circulation; and ultimately the lump in the ham disappears altogether, or remains as only a small, hard, non-pulsatile tumour. If the artery is ligatured, the effects are the same—the main channel alone is obstructed, and the flow through the collateral vessels being unimpeded, soon causes a stream of blood to pass into the main vessel below the ligature. The usual change produced in the aneurism is a gradual deposit of fibrin in concentric laminæ until its cavity is occluded; and then the process spreads to the artery, where the growing obstruction quickly produces stasis, and the formation of a true blood-clot. This clot becomes organised, and the fibrin in the aneurism undergoes more or less complete absorption. In some cases, as already mentioned, during compression of the artery above, blood has coagulated *en masse* in an aneurism. But this is not usual, nor is it aimed at in applying this kind of compression.

Compare with this a case of popliteal aneurism, treated successfully after Dr. Reid's plan. The elastic bandage is applied firmly from the roots of the toes up to the lower part of the ham, then it is intermitted or applied only very lightly over the aneurism, while above the tumour it is applied very firmly to about the middle of the thigh, and then, if there is still any trace of pulsation in the sac, the elastic tube or tourniquet is applied tightly until it ceases. After about an hour this apparatus is removed, and the aneurism is then found to be firm and not pulsating; compression is applied to the artery at the groin for from six to thirty-six hours, during which the tumour shrinks; pulsating vessels are soon detected around the knee-joint, and quickly the aneurism disappears altogether. In this account I have purposely omitted the many lesser details to be alluded to further on. The first thing to be noticed is that the compression is applied not to one spot of one vessel only, but to all the vessels of the limb in their whole length, except just at the site of the aneurism. Experience of the elastic bandage in amputations and excisions shows that it expresses the blood from all the vessels of the part to which it is applied, and arrests all circulation through them, both direct and anastomotic.

Indeed, it is evident that its effects are first and mainly felt by the capillaries and smaller vessels, and that the larger arteries are the latest to be emptied by the pressure. In the case we have supposed, then, the vessels of the foot, leg, and lower half of the thigh, will be emptied more or less completely, while those in the ham and around the knee-joint will be full of blood; but as the circulation both above and below the ham is entirely arrested, this blood will be stagnant, and no emptying of the full vessels or movement of the blood will be possible. The primary effect, then, of the bandage is to distend the aneurism and adjacent vessels with stagnant blood. The blood in the aneurism coagulates, and the clot spreads into the artery; but the blood in the other vessels—although stagnant and exposed to many of the same influences—remains fluid. If the bandage is applied as we have described, laminated fibrin cannot be formed; the fibrin must enclose in its meshes the corpuscles, and form a true clot. The fact that this treatment causes absolute stasis of blood in the aneurism and adjacent vessels for a certain time, which allows of coagulation of the blood *en masse*, distinguishes it from every other treatment for external aneurism yet introduced. In successful cases the part of the clot in the artery organises, but that in the aneurism is for the most part absorbed. We may say, then, that in the cure of the aneurism treated by this method there are four separable steps:—(1) the stasis of the blood in the aneurism and adjacent vessels; (2) the clotting of the blood in the aneurism and adjacent artery; (3) the organisation, and (4), later on, the absorption of the clot. We must, therefore, endeavour to ascertain the circumstances causing, favouring, or preventing each of these events.

To cause the stasis of the blood—a purely physical effect—the only thing necessary is the proper application of the elastic bandage. But it is important to notice the different parts played by the bandage below and above the aneurism. In the case of a popliteal aneurism the elastic bandage or tourniquet, applied sufficiently tight around the thigh, will, of itself, cause stasis in the whole of the limb below; and in Cases 7 and 56 this simple method was adopted. In all similar cases it is the bandage above the aneurism that is the sole means of causing stasis in the sac, and is therefore the essential part of the application. What, then, is the use of the bandage below the sac? It empties the vessels of the part, forcing blood into the general circulation, and so causes the limb to be practically bloodless during the application of the bandage above; and it in no way affects the stasis, or the condition of the aneurism, except that it favours its distension when the pressure is applied above. It has been suggested by Mr. C. Heath that distension of the aneurism may aid the coagulation of the blood; but he has not adduced any evidence in support of the suggestion, and it would seem that any such supposed advantage is more than compensated by the risk of rupturing a very thin sac. It may be, however, that circular compression of the limb just below the aneurism may cause the stasis to be more perfect than it would otherwise be, for without it the dilatation of the vessels from vasomotor paralysis, owing to the cessation of the circulation, might cause gentle and slight ebbs and flows in the blood. Again, the bandage below the sac certainly prevents capillary rupture from the compression of the limb above, such as has been shown to occur around the aneurism. A simple modification, which answers these purposes, and without the disadvantages of keeping the whole limb bloodless and of forcing an undue quantity of blood into the general circulation, is to apply the elastic bandage to the limb only immediately below the aneurism and while the part is horizontal. This will not over-distend the vessels below, and will prevent the least emptying of the aneurism. If the upper bandage is then applied to the limb in the vertical dependent position the aneurism will be distended, if that be desired.

Another way of obtaining this stasis in the aneurism was practised in Cases 60, 65. It consists in applying the elastic bandage quite up to the sac, and then, with the finger or some mechanical appliance, compressing the artery close above the sac. In this method it is the lower bandage that is the important and essential part. The compression of the main trunk does not cut off the supply of blood through the various anastomosing channels, but all such anastomotic circulation is entirely prevented by the compression of the vessels below the sac. The compression of the main artery in such a case is of use only in entirely preventing the shock of the ventricular systole, which would otherwise be transmitted to the blood in the aneurism, and cause some commotion in it. It is a reversal in this particular of the common method. There the compression above the aneurism is the essential, that below is accessory only; here the compression below the sac is essential, and that above accessory. This method is employed in aneurisms so placed that the elastic bandage can be applied up to, but not above, the aneurism, and when the main artery can be readily controlled close above the sac; these conditions are exemplified in aneurism at the apex of Scarpa's triangle (case 60). The same method has been employed for an aneurism of the external iliac artery (No. 65); Lister's tourniquet being applied to the common iliac artery. In this case, however, although the result was successful, it is to be noted that the anastomotic circulation through the epigastric and circumflex iliac arteries was not in any way controlled. Dr. Reid's treatment is, of course, inapplicable to aneurisms of the neck, for the special kind of compression cannot be employed in this region.

Then comes the question—*Why does the blood coagulate?* What condition is it that leads to the escape from the corpuscles of the ferment that has the power of exciting the union of fibrinogen and fibrinoplastin? It is not the stasis of the blood in the living vessels; it is not the cessation of all circulation of blood through the vessel-walls themselves, for Mr. Lister's experiments have shown that blood can be kept fluid for a long time in veins removed from the body, even when open to the air; indeed, it is evident that the cause of the coagulation is not in any condition of the vessels themselves, for the stagnant blood does not coagulate in the veins, or in arteries other than the one diseased. Further, it has been found in some cases that on removal of the bandage the aneurism has been only partially consolidated, with lessened pulsation, while in others the aneurism has become solid, but has exhibited pulsation transmitted from the connected artery; but in no case has it been noticed that the blood in the aneurism has remained fluid, but free from pulsation owing to the thrombosis in the artery. And hence we may conclude that in all cases the coagulation starts in the aneurism, and that the clot may partially or completely fill the aneurism, or extend into the artery; and that the clotting never commences in the artery itself. When the blood is in motion the flow through an aneurism is more irregular and turbulent than through an artery, and this may have some influence upon the formation of fibrin; but when the blood is stagnant, as in the case in point, the only differences between the two are those of contour and structure. As an aneurism usually is in shape a nearer approach to a globe than is an artery, the surface contact of the contained blood is proportionately less. This, then, will not explain the coagulation in the aneurism. We must, therefore, look to the differences in the structure of the two; the absence of endothelial lining in the aneurism, its want of perfect smoothness, or the presence of fibrin on the inside of the sac, or some special vital reaction of the tissues of the sac upon the blood, may one or all be regarded as tending to start coagulation. Clotting, once started, would tend to spread through all the stagnant blood, and therefore into the artery, but rapid circulation through the artery would entirely prevent it there; we see this illustrated in the case of a ligatured artery, in which the clotting starts at the rupture of the two inner coats, and spreads up through the stagnant blood, but is sharply limited by the mouth of the first large branch—*i.e.*, where the blood is flowing rapidly.

The main fact in regard to the mode of permanent cure of an aneurism is, that the blood-clot in the artery becomes replaced by a living mass of fibro-cellular tissue, while most or all of that in the aneurism becomes dried, discoloured, and partly absorbed but not organised. How is this difference in the destiny of two parts of a continuous and homogeneous blood-clot to be accounted for? If no other evidence were at hand, this fact alone would demonstrate that the changes a clot forming in the living body undergo depend not so much upon the clot itself as upon the condition of the tissue in which it lies. This is well illustrated by what we know of the fate of a thrombus in a healthy artery as compared with that of a similar thrombus in an artery the seat of advanced atheroma—the one organises into fibrous tissue, the latter fails to organise at all. Whatever view may be held as to the exact part the constituents of the clot play in its organisation, whether the white corpuscles of the clot be regarded as taking an active share in the cell development, or the clot be regarded merely as a suitable nidus for organising lymph (which seems more probable), it is equally clear that the process depends upon the condition of the tissues surrounding the clot. If these tissues are healthy and vascular, plastic lymph is readily effused and infiltrated into the clot; but if they are diseased, and especially if they are non-vascular or devoid of all active vital properties, the lymph is not effused, the clot is not organised. It is, however, quite consistent with this view also to hold that the nature of the clot itself may exert some influence upon the process of its organisation. But the blood-clot only organises through the agency of lymph effused from the surrounding tissues.

Applying these considerations to the case in point—to a clot filling an aneurism and the adjacent part of the artery—it is evident that the nutritive conditions of the sac and the artery may be and usually are different. The artery is entirely composed of living tissue, and it is only the extreme degrees of atheromatous disease that are incompatible with the due organisation of a thrombus; provided that an arterial thrombus be undisturbed, and not septic, and that destructive inflammation or gangrene do not attack the wound, it is of extreme rarity that it does not become organised. But in the sac-wall we have quite other conditions. Very often there is a more or less thick lining of laminated fibrin—lifeless, hard, resistant matter, with no power of effusing lymph, but considerable power of preventing its transudation. The sac-wall proper may be of various composition, but is generally formed either of the outer coat of the artery, or of condensed fibrous tissue; and in either case, from the stretching to which it is subjected, its supply of blood is interfered with; so that the sac of an aneurism is by no means adapted for the effusion of plastic lymph into a clot within it. Hence, under favourable circumstances, the thrombus in the artery organises into fibro-cellular tissue, while the clot in the aneurism does not. In other words, *the permanent cure of the aneurism is brought about by organisation of the thrombus in the artery rather than by any change in the aneurism itself, the clot in the aneurism being of use mainly as a means of securing a thrombus in the artery.* Of course, so soon as the artery is obstructed the tumour springing from it ceases to be an aneurism proper. Strongly supporting this view is the circumstance that, as shown in the Table of Cases, in every instance of popliteal aneurism where the state of the vessels about the knee-joint after the cure is mentioned, it is stated that the anastomosing vessels were enlarged, and evidently carried on the circulation in place of the plugged popliteal trunk; in ten cases this is distinctly stated. No case of cure is recorded of which it is stated, or can be inferred from the report, that the aneurism was occluded without coincident obliteration of the artery. The clot in the aneurism does not organise unless immediately surrounded by vascular tissues; where fibrin in any quantity intervenes, its organisation is delayed or entirely prevented, the clot then shrinks by its inherent power of contractility, the serum is absorbed, and the dried remains to undergo absorption or remain behind as a soft friable mass.

The process of cure by this treatment depends, then, first, upon clotting of the blood, which commences in the aneurism, and spreads into the artery; and, secondly, upon the organisation of the clot in the artery. The clotting of the blood, which is an act of death, commences in the aneurism because its walls are deficient in some as yet not understood vital power; but the organisation of the clot in the aneurism often fails for a closely allied reason; and occurs in the artery because its walls are vascular and their vitality is not seriously interfered with.

Conclusions.—There are some few conclusions that it may be well to state here. The first of these is that Dr. Reid's method of treatment has hitherto been so successful, and in so short a time, is so easily applied, and its success or failure so readily recognised, that it is well worthy of adoption in suitable cases. Its alleged dangers and disadvantages we have seen to be, in most instances, exaggerations; and there are none which may not be obviated by a judicious selection of cases and a careful attention to the details of the treatment. I would wish again to emphasise the fact, that cure of an aneurism by this method is not obtained in the same gradual manner as by most other methods, but is rapid; it is never partial—a mere improvement—but is complete; and it depends on the organisation of a thrombus formed in the diseased artery.

In the selection of cases suitable for this treatment, the most important point is an otherwise healthy blood-vascular system. Wherever possible, preliminary treatment should be employed, with the view of increasing the coagulability of the blood; the most successful treatment of this kind is a dry albuminous diet, and the administration of iodide of potassium in full doses. The bandage must be applied so as to secure complete stasis in the aneurism and adjacent artery; but this should be done with the least possible disturbance of the general circulation; and, therefore, the blood should not be expressed from the whole of the limb below the aneurism, but only from the part immediately below the tumour. Stasis should be kept up for one and a half hours. If the aneurism is at the end of that time solid, the bandage should be removed, and the main artery of the limb compressed by the finger or tourniquet for six to twelve hours. If the blood has not clotted in the aneurism, such compression should equally be employed. Wherever the aneurism is of large size, and, especially, if rapidly growing, the elastic bandage should be carried lightly over the tumour. When this treatment fails, the artery above may be ligatured with as good a prospect of success as if no previous treatment had been tried.—*Mr. Pearce Gould's Pamphlet on the Rapid Cure of External Aneurism; Kolckmann, London,* 1882.

ALIMENTARY CANAL.

48.—ON EXCISION OF THE ENTIRE TONGUE BY WHITEHEAD'S METHOD.

By Frederick Treves, F.R.C.S., Assistant-Surgeon to, and Senior Demonstrator of Anatomy at the London Hospital.

Two cases are recorded in the Lancet (*vide* the two following articles)—each by a separate surgeon—of removal of the tongue by the method introduced by Mr. Whitehead, of Manchester. This operation is so well known that it is merely necessary to observe here that it consists in removing the entire tongue through the mouth with scissors only, and without any preliminary deligation of vessels. This method is an important departure from the operation of Billroth, who, while excising the tongue in a manner practically identical with that advised by Mr. Whitehead, first secures the two lingual arteries in the neck. In both the recorded instances just alluded to, a good recovery followed; but in criticising the operation one of the surgeons—Dr. Andrew Marshall—asserts that "apparently no method could be easier, safer, or more satisfactory;" while the other—Dr. George Elder—expresses his intention of not again repeating the operation, but of adopting instead the procedure of Billroth. From these comments it would be obvious, in the absence of any other evidence, that a great difference of opinion may exist as to the value of Mr. Whitehead's operation, and I imagine that the truth in this matter can best be arrived at by an accumulation of cases from different sources. I venture, therefore, to detail the particulars of a case in which I recently removed the entire tongue by Mr. Whitehead's method. In the first place it must be stated that the main and perhaps the sole objection to this method is founded upon the matter of hemorrhage. If the question of probable bleeding be put aside, this operation would appear to be in every way excellent. By its means the tongue can be removed entire with ease, with precision, and with rapidity. The neighbouring parts are but little disturbed, and all the evils incident to the crushing or burning of tissues (as involved in the use of écraseurs) are avoided. Apart from this simple question of bleeding, I believe the only serious objection to the operation is that raised by Prof. Stokes, who asserts that it is apt to be followed by septic infection. This objection has, however, been, I think, fully and satisfactorily met by Mr. Whitehead himself. As regards this question of hemorrhage the most conflicting statements have been made. In Dr. Marshall's case above noted, "no bleeding of the slightest consequence occurred during the process until the lingual arteries were cut," while in Dr. Elder's case the loss of blood was very considerable, and, indeed, so serious as at one time to threaten asphyxia. Mr. Whitehead, in his account of the operation in the Congress Transactions, says that "the danger from hemorrhage is simply *nil;*" he further speaks of the bleeding as "trifling," and as "easily controllable." Mr. Lund, on the other hand, in his excellent account of the operation, states that the bleeding may be "very profuse," and *à propos* of the subject of loss of blood speaks of the "regulation pint," although he does not infer that that amount is ever lost in this particular operation. In detailing the following case, therefore, I shall deal almost exclusively with this matter of hemorrhage.

A man, aged fifty-three, a clerk, was admitted into the London Hospital, under my care, on Dec. 3rd, 1881. The middle third of the left side of the tongue was occupied by an epitheliomatous mass that had ulcerated extensively and had been noticed about five months. The floor of the mouth, and indeed the under surface of the tongue itself, were free from implication in the disease. There was no gland affection. The ulcer had never bled. The patient had never had syphilis, had been a careful liver, and, apart from the tongue affection, was in remarkably sound health. He presented no evidences of atheroma. The tongue was removed on Dec. 6th. Chloroform was given, Mason's gag was used, and the tongue was drawn forward by the double ligature, as advised by Mr. Whitehead. An ordinary pair of straight scissors was used. The frænum was first divided, and I then began to cut the mucous membrane and connective tissue in the floor of the mouth, between the tongue and the jaw, after the plan laid down by Mr. Whitehead, and more fully detailed by Mr. Lund. I had not, however, got very far before blood began to flow very vigorously from the wound. This should be at once checked, Mr. Whitehead says, by steady pressure for a minute with a sponge. This measure, however, had no effect on the bleeding, which continued in a very determined manner, and each snip made with the scissors added materially to the amount. Had I followed Mr. Whitehead's plan strictly I should not have proceeded with the operation until the bleeding had been checked, and having divided the mucous membrane connections of the tongue I should then have divided the muscular attachments, cutting first one lingual artery and then the other, and securing the first vessel before the second was divided. Lastly, just before the separation of the tongue was effected I should have passed a loop of silk through the remains of the glosso-epiglottidean fold, so that the epiglottis may have been, if necessary, drawn forwards.

What was actually done was this: The hemorrhage being considerable, and apparently unaffected by pressure and by drawing the tongue forwards, the best course appeared to be to remove the entire tongue as rapidly as possible, and to then deal with the bleeding when the organ was out of the way. Free hemorrhage from a partly removed tongue appeared to be more serious than still freer hemorrhage from the floor of the mouth with the tongue out of the way. Moreover, practice on the subject had shown that the entire tongue could be removed with scissors in about a minute, and I imagined that bleeding, even if severe, for that length of time could not lead to very grave results. I therefore at once removed the entire organ with the greatest possible expedition. When removed, blood was seen to be pouring from the cut surface of the floor of the mouth for the moment during which that surface was of necessity exposed. As the tongue was taken out, a large sponge was at once thrust into the mouth and pressed firmly down. The patient, having a little recovered from the anæsthetic, was able to clear his throat to some extent. The next point was to secure the linguals. The sponge was kept pressed down to the floor of mouth until it was considered that the oozing from the surface might have been checked. On its removal, however, blood welled from the general surface of the wound as freely as ever. The lingual arteries were readily seen as the blood from them was spurted directly out of the mouth. One of these vessels was immediately picked up with the torsion forceps, but with one twist of the forceps the end of the artery came off, and the bleeding, of course, recurred. Although this occupied but a moment, the hemorrhage from the general surface of the wound was most profuse, and the blood by running down the throat threatened to produce asphyxia. The sponge was reinserted and kept firmly applied for a still longer period. On its removal blood still poured out from the wound surface, a clamp forceps was put on one lingual and an attempt made to ligature the other; but, although the ligature was applied with the greatest care by Mr. M'Carthy, who kindly assisted me, the end of the vessel came off with the ligature when the forceps was removed. The sponge was again applied. The bleeding so far had been terrific, and it is particularly to be noticed that it consisted of a general oozing from the cut surface, and not from the pumping of individual arteries. Had there been no oozing and the mouth been kept well open not a drop of blood need have run down the throat, even when the linguals were unsecured, because the blood from the vessels was directed straight out of the mouth. It was this general oozing, and not the bleeding from the linguals, that caused the severe hemorrhage in Dr. Elder's case; and it is the possibility of this form of hemorrhage that appears to be so serious a feature in this operation. Several attempts were made to ligature the linguals, but in each case the silk cut through. During these attempts, the oozing from the general surface occurred, with scarcely diminished vigour; and the hemorrhage, indeed, was so serious that it would have been no matter of surprise if the man had died on the table. Ultimately the vessels were secured by passing a tenaculum beneath them, and by including in the ligature a good deal of the adjacent soft parts. In spite of long-continued pressure, the oozing of blood did not cease until the man was almost pulseless and ice had been for some time applied to the part. The brittleness of the lingual arteries and the softness of the structures adjacent to them were remarkable, and are difficult to account for. The condition of these parts served to render torsion utterly out of the question, and the ordinary ligature almost impossible. It would certainly be unfair to ascribe the hemorrhage in this case to Whitehead's operation, inasmuch as in some minor points that procedure was departed from. But I am strongly of opinion that the results would have been still more grave had I closely followed the details of Mr. Whitehead's plan. If the floor of the mouth is well pressed up from without, it is not very difficult to pick up the lingual vessels after the tongue has been removed; but to have endeavoured in this case to have picked up one or other of these arteries when the tongue was only partly excised, and when free oozing was going on, would have been to still more seriously complicate the operation. Had the operation been discontinued until the first bleeding had been arrested, I imagine that the proceedings would have been indefinitely and dangerously prolonged; and considering the difficulties in the way of both torsion and the ordinary ligature, I believe that an attempt to secure one lingual artery, while the tongue was only in part removed, would have ended in failure. The present case might be exceptional, and it would be unjust to condemn an operation like that of Mr. Whitehead's upon the circumstances of a single case; but for my own part, in repeating the operation, I shall certainly adopt Billroth's precaution of first ligaturing both linguals. It would, I think, be safe to conjecture that the general oozing was, for the most part, from vessels derived from the lingual arteries; and it is stated, moreover, that the operation is comparatively bloodless when the preliminary ligature is adopted. As to the future of the present case, the man recovered from the effects of the operation better than could have been expected. There was no return of hemorrhage. For two days and a half he was fed solely by peptonised enemata. The mouth was kept frequently washed out with Condy's fluid in iced water. On the third day he was fed through the nose, and the enemata were discontinued. He gained strength rapidly, and appeared to be doing remarkably well, until December 12th, when symptoms of pyæmia, ushered in by a severe rigor, became evident; and of this blood poisoning he very rapidly died. A post-mortem was refused by the friends.—*Lancet, April* 22, 1882, *p*. 641.

49.—EXCISION OF TONGUE BY WHITEHEAD'S METHOD.

By GEORGE ELDER, M.D., Surgeon to the Women's Hospital, Nottingham.

On October 27th, 1881, I saw, in consultation with Dr. Wallace, of Arnold, Mr. K——, a publican, aged fifty years, suffering from a ragged, dirty-surfaced fissure of the tongue, with indurated edges, situated on the left side, about the junction of the middle and posterior third. There were also several patches of psoriasis linguæ; but no implication of neighbouring glands. The condition of the tongue rendered speaking and eating very painful and difficult. No doubt the causes of the disease were—(1) the existence of a rough-surfaced tooth stump, (2) his fondness for spirituous liquors, and (3) intemperate smoking; occasionally as many as twenty cigars being smoked in a single day. No history of syphilitic infection could be elicited. The general health had not suffered, and although Dr. Wallace had fairly tried potassium iodide, it was resolved to give mercury locally and constitutionally a trial before counselling extirpation of the tongue. The offending stump was also removed, and smoking and drinking alcoholic stimulants stringently forbidden. I may say that prior to seeing me the patient had been told by his medical attendant how grave the case was, and the very great improbability of anything short of removal being of any service; still, as the glands were unimplicated and the general health kept good, combined with the fact that he was under constant medical supervision, the medicinal treatment above referred to was thought worthy of a further trial. He was again brought to me in Dec., worse locally, inasmuch as the original sore had become deeper, dirtier, and very prone to bleed. Its edges also had become more decidedly indurated and raised, and on the right side of the tongue, almost in a line with the other, there had appeared since his last visit a hard, nodular swelling, about the size of a small nut, which at one point gave signs of disintegration. Under the mercurial treatment and careful living the disease at first seemed stationary; but for a week or two before coming to see me his general and local condition had rapidly become worse. Still there was no glandular infection, and excision of the whole tongue was proposed as holding out a good chance not only of prolonged life but also cure. To this the patient consented, and on Dec. 18th I performed Whitehead's operation, assisted by Mr. Joseph White and Drs. Wallace, Truman, and McMillan. Whitehead's operation consists of cutting through the attachments of the tongue, by successive, short cuts, with a curved pair of scissors, the organ being drawn forwards and upwards by means of stout whipcord. Chloroform was the anæsthetic used, and prior to the operation the head was well elevated.

During the operation there was very considerable loss of blood, seemingly not so much from division of the blood-vessels as from the general surface of the wound. The position in which the tongue was of necessity held, combined with the flow of blood, so impeded respiration towards the termination of the operation as to threaten asphyxia; but, fortunately, at this juncture the patient became partially conscious, and assisted in clearing the throat. A few more cuts with the scissors completed the operation. The right sublingual artery was the only vessel which required ligation, and after removal of the tongue the bleeding was soon brought under control by the presence of sponges and subsequent application of perchloride of iron. For several days after the operation nothing beyond iced milk was swallowed, food being administered by enemata. At no point of the convalescence did the patient's condition give any anxiety. His recovery was rapid and uninterrupted; so much so that within three weeks after the operation he was walking about, talking, smoking, and drinking pretty much as before its occurrence. His sense of taste is unaffected, and when his food is minced he has no difficulty in swallowing it.

There cannot be a doubt about the comparative simplicity of this operation in excision of the whole tongue. By means of the scissors one can make absolutely sure of cutting away the entire organ; a point which is not so certain or easy of attainment by means of the écraseur or galvano-cautery. Its advantage over ligation is self-evident. Although my experience of Whitehead's operation has been confined to this case, still the danger from the bleeding seems to me to be greater than he is willing to admit, and on repeating the operation I shall first ligate the lingual arteries as recommended by Billroth, assured that in doing so the danger to the patient is minimised.

I may say that microscopical examination of the tongue after its removal proved the malignant nature of the disease.—*Lancet, Feb. 25, 1882, p.* 306.

50.—EXCISION OF TONGUE BY WHITEHEAD'S METHOD.

By **Andrew Marshall**, M.D., Preston.

Thomas H——, aged fifty-six, a gardener, from Fulwood, was admitted into the Preston and County of Lancaster Royal Infirmary, under my care, on November 16th, 1881, with cancer of the tongue. He gave the following history of his case:—He had good health until five months ago, when he felt a lump in his tongue. He suffered great pain in it from the first. The pain was constant, sometimes very severe; of a lancinating character, shooting up towards the ear. He attributed the disease to the irritation produced by some decayed teeth with sharp edges. These were removed, and the pain subsided considerably. He was accustomed to smoke a short clay pipe. On admission the growth was found to occupy the right middle third of the tongue. It was very hard and ulcerated, and presented an unhealthy appearance. There was no evident implication of adjacent parts. Articulation and deglutition were somewhat affected. The patient said he never had syphilis. His grandfather died of cancer of the jaw. The case was considered a favourable one for excision, and removal of the whole tongue was recommended. The nature, risks, and probable results of the operation having been explained, the patient decided to have it done. Accordingly, on Nov. 23rd, chloroform having been administered, the mouth widely opened and fixed in that position by Mason's gag, and a double ligature introduced through the tongue, by which to hold it during the operation, the frænum was cut across by a long and strong pair of straight scissors with rounded ends. The mucous attachments of the tongue to the jaw and the pillars of the fauces were next divided by the same means, and subsequently the muscles uniting it to the floor of the mouth were similarly severed by snipping them, first on one side and then on the other. No bleeding of the slightest consequence occurred during the process until the lingual arteries were cut. Each of these vessels spouted forth blood, which was immediately arrested by pressure with the forefinger, until the small quantity of blood in the mouth was wiped away with a sponge held in forceps, and then the vessel was at once and easily seized with a pair of torsion forceps and twisted, thus being satisfactorily and permanently occluded. A ligature was applied through the remaining attachment of the tongue in front of the epiglottis, and this connecting-band being clipped through in front of the ligature, the tongue was removed in its entirety without any trouble whatever. The operation occupied four minutes and a half, and the total loss of blood was estimated at rather less than one ounce. The patient was fed entirely by enemata for the first three days. Speaking was prohibited for one week. The mouth was cleansed from time to time by a weak solution of Condy's fluid. Convalescence was rapid. The patient went out of doors on Dec. 4th, and was discharged on the following day at his own request, apparently quite well.

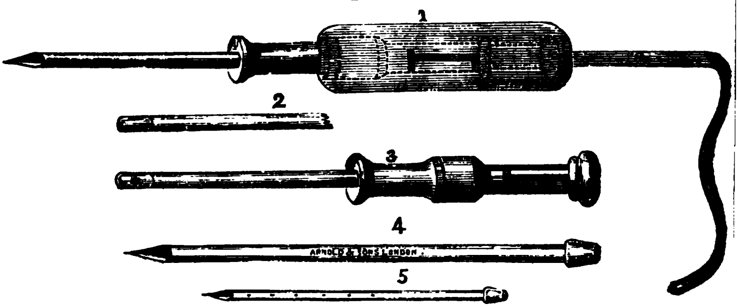
I began this operation with great dread, expecting profuse bleeding to occur, filling the mouth, flooding the fauces, probably entering the trachea and threatening to suffocate the patient, if not actually doing so. At the very least I anticipated a somewhat prolonged operation on account of the stoppages that might be necessary in order to arrest the hemorrhage. But nothing of this kind took place. The bleeding, insignificant in amount, was easily stopped by pressure with sponges, except in the case of the linguals, which were secured without any difficulty. Apparently no method could be easier, safer, or more satisfactory than this of performing what is usually described as an extremely difficult and dangerous operation. By it the most complete and accurate removal of the whole tongue may be effected. It is a much less severe operation than Mr. Syme's, and has the advantage of not leaving any external marks. Mr. Whitehead claims for it a greater degree of safety than can be attained by any other means of equal efficacy, and the statistics he gives seems to justify his claim.

Judging from this one case it appears to me that Mr. Whitehead's plan only requires to be known and tested in order to supersede all the other methods employed for removing the tongue.—*Lancet, Dec.* 25, 1881, *p.* 306.

51.—TAPPING, AND A NEW ANTISEPTIC TROCAR.

By John Ward Cousins, M.D.Lond., F.R.C.S., Surgeon to the Royal Portsmouth Hospital.

It was an old-established rule in surgery—long before the introduction of antiseptic treatment—that every precaution ought to be taken in all tapping and exploring operations, to prevent atmospheric contact with the interior of the cyst or cavity. The old-fashioned trocar is not well adapted for this purpose. Some years ago I used a flexible tube attached to the canula, and through its side I introduced the trocar, so that on its withdrawal the perforation was closed by the elasticity of the tube. This, however, was a very clumsy proceeding. The syphon trocar invented by Mr. Charles R. Thompson, of Westerham, has been very generally employed of late years, and a modification of it has been introduced by Mr. Spencer Wells for the purposes of ovariotomy. The protection against the admission of air depends wholly upon the action of the piston, which must fit the canula accurately, and also be well greased, so that its action may be easy and secure. Sometimes this instrument is furnished with a stopcock, which can be turned upon the retreating trocar, and this certainly affords additional protection. In my experience, however, piston trocars are very liable to derangement. The new trocar which I desire to introduce to the notice of the profession is a very simple and handy instrument. The special feature in its construction consists in this, that it is perfectly air-tight, and can be opened and shut within an india-rubber case (see Figs. 1, 2, and 3). It is composed of two metal tubes; the outer carries a pen-shaped lance, and the inner is round at the point with a lateral opening. By a bayonet joint the trocar is opened and shut, at the same time the point of the lance is protected from injuring any internal part. The orifice and joints are introduced into the enlarged end of the india-rubber tube, which is slipped over a metal block, and then secured by a ring, and this serves for a handle to the instrument. It is adapted for all kinds of tapping; it can be used also for exploring or injecting purposes, or it can be very readily attached to the exhausting apparatus of the aspirator.



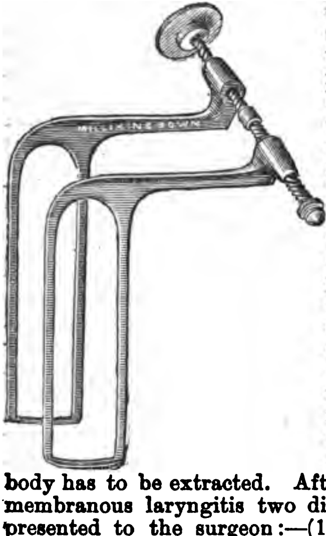
The trocar is manufactured by Messrs. Arnold and Sons, of West Smithfield, in three convenient sizes. I have now employed it in every variety of operation, and with special advantage in cases of paracentesis thoracis. The opening and closing action within the india-rubber case is very readily accomplished; and the necessary movement can be easily performed by anyone after a little attention to the construction of the instrument. Fig. 4 represents a simple form of exploring tube, to which an india-rubber bag can be adjusted. The capillary drainage-tube (Fig. 5) is intended for the treatment of anasarca, and it can be left in the subcutaneous tissue without any risk of injury to the deeper parts, and its position can be altered without another puncture of the skin.—*Lancet*, *Nov.* 26, 1881, *p.* 909.

52.—MECHANICAL TREATMENT OF CROUPOUS MEMBRANE AFTER TRACHEOTOMY.—TRACHEA DILATOR.

By C. H. Golding-Bird, M.B., F.R.C.S., Assistant Surgeon to Guy's Hospital.

In membranous laryngitis after tracheotomy has been performed, the case resolves itself, as far as the surgeon is concerned, almost entirely into one of foreign body in the trachea and bronchi, that is if the membrane has already reached so far down; if not, then the dyspnœa will be for the time quite relieved by the operation, but in due course the disease having reached the lower parts of the windpipe, symptoms of a foreign body obstructing respiration again appear. Usually on the opening of the trachea a suitable silver tube is at once inserted, as much with the idea of stopping blood getting into the trachea as of relieving the dyspnœa. For some time after, the child coughs up clots and blood-stained mucus, with perhaps some membrane, whilst the greatest anxiety is felt lest the tube should become blocked, and careful cleansing by feathers, &c., has consequently to be carried out at very frequent intervals; and the patient often does not even then breathe satisfactorily. Such a mode of procedure is *mechanically* wrong, to say the least of it. Everything save air and a minute amount of mucus in the windpipe is a foreign body; and such are the blood and mucus, as well as the membrane, in every croup case. These can scarcely be expelled *viâ* the glottis, for that is already blocked by membrane, while the chances of their finding their way out through a lateral, rigid, and bent tube, the end of which hangs freely in the trachea, like the stem of a funnel in a wide-mouthed bottle, are very remote. If the tracheotomy tube were of the same calibre as the trachea itself, expectoration would be more easy; but it is not only smaller of necessity, but hangs (or should hang) freely in the lumen of the trachea, and with some degree of mobility. That blood and mucus—let alone the more solid membrane—will be readily forced by natural efforts exactly through the mouth of this tube is unlikely, clinical experience shows; and then these foreign matters collect around or below the tube, and are sucked backwards and forwards for long before they hit the right path; while the futile efforts of a piece of membrane to make its exit is announced by a valve-like flapping noise, familiar to all who have had much to do with tracheotomy in croup.

Until, therefore, the trachea has been cleansed as far as possible from all foreign bodies, the tracheotomy tube should not be inserted, but other means should be adopted to keep the tracheal wound patent. In eight cases during the last nine months I have acted upon this rule, employing at first the ordinary German nose speculum, as doubtless others have done, instead of a tracheotomy tube, the blades of the instrument being inserted in the tracheal wound, and then screwed open. Although fairly self-retaining, there were many reasons against its being perfect for the purpose, and so the tracheal dilator here figured has been designed. It is inserted, with the blades screwed together, into the trachea, the handle lying against the top of the sternum; on rapidly turning the screw, the blades separate to the required extent, and their shape renders them self-retaining. If desired, tapes can be tied to the blades where they join their handles, and fastened round the neck, but this is not often requisite. The blades retain their parallelism even when fully open, as they do not work from a hinge but travel along the double screw. The same sized dilator does for all cases, whether infants or adults. In any case it may take the place of a tracheotomy tube on an emergency, even when no foreign body has to be extracted. After tracheotomy in every case of membranous laryngitis two distinct mechanical problems are presented to the surgeon:—(1) The establishment of a free passage through which membrane, &c., may be expelled by natural efforts; or, this failing (2) its removal by artificial means. In either case, as a preliminary a sufficiently free opening into the trachea must be made—five-eighths of an inch even in infants, and proportionately longer in older persons.



This done, the following steps fulfil as nearly as possible the above requirements:—

(*a*) At once insert, with the left hand, into the tracheal wound, the blades of a pair of dressing forceps, and then open them as widely as possible. Free expectoration of blood, mucus (and perhaps membrane) will follow, and these can be rapidly sponged from the lips of the wound. A clear passage being thus insured, expectoration and coughing are rapidly over and full inspiration restored, while all blood is brought up from the air passages within a few minutes.

(*b*) To determine the presence of membrane in the trachea, *i.e.*, below the glottis, reflect a strong light into the wound from a mirror on the operator's forehead. If there is no membrane the posterior wall of the trachea may be seen pink and moist, and looking very much like the outer wall of the œsophagus. Small pieces of sponge held in dressing forceps can be used to clean it from bubbles of mucus. If healthy it may be taken for granted that there is no membrane below—the formation of membrane proceeding from above downwards. If an inspection be now made in an upward direction through the wound, membrane will perhaps be seen just entering the trachea from the larynx. The operator is at present below the obstruction.

(*c*) To maintain the tracheal opening patent, a tracheotomy tube can be now inserted, until evidence of membrane in the trachea is forthcoming; but supposing this stage now reached, or to have existed at the time of the operation, a tube is best dispensed with. Instead of dressing forceps or tube, insert the tracheal dilator into the trachea itself, not only into superficial tissues, and dilate until it holds firmly; the least possible opposition is thus offered to naturally expulsive efforts.

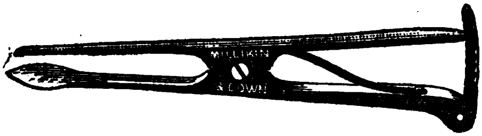
An inspection of the trachea will now show either a layer of characteristic membrane, or else the mucous surface will have lost its pink colour, be cloudy and white; when this is seen it may be taken as a sure sign of coming membrane. In cases where the membrane has been stripped off and the normal appearance as far as possible restored, this white cloudiness will be always a sure precursor of a fresh formation.

(*d*) To remove membrane if seen. If natural efforts have failed the surgeon should (lighted still by the mirror on forehead) remove with forceps (for example, aural polypus forceps) all that can be seen, or that is coughed up to the opening. Membrane may be now searched for below the wound. Minute pieces of sponge held in curved forceps, or feathers can be passed down; but I prefer for this purpose a brush like a rather large pipe cleaner, on strong but flexible steel wire. I at first used Maw's feeding-bottle brushes, but they are very liable to break, and therefore had the stronger ones made. Before use they should be put into hot water. They may be passed in a child three inches from the wound—that is, well into the first division of the bronchi, and if rotated sharply at the moment of retraction, will detach and draw up often large pieces of membrane. When mucus rather than membrane obstructs, feathers and minute sponges are preferable. If the air passages are thus rendered free, the patient can be left again to the nurse with instructions to mop out frequently with small pieces of sponge, held in artery forceps, anything coughed up to the wound; and also, by reflected light, to pick out any visible pieces of membrane. At the same time the usual adjuncts of alkaline and other sprays should be employed. Hot flannels can be placed over the dilator. There is less anxiety to the surgeon now, for there is no tube to get blocked or to be cleaned by the nurse; for the dilator need never be touched even for days, and as the wound granulates it is embedded and held more and more firmly. It is perfectly comfortable, and seems to give no pain; it causes no irritation, and except in one case, not under my care, where a small abrasion was seen after death of the mucous membrane of the trachea, it has left no mark of its presence. Automatic retractors for the soft parts, during the operation, are recommended by some; but to urge the systematic employment of tracheal dilation instead of a tracheotomy tube, as long as any foreign body, of whatever kind, has to be got rid of, is the object of this communication.

[Mr. Golding-Bird's Tracheal Dilator is manufactured by Millikin and Down, 3, St. Thomas' Street, London, S.E.]—*Lancet, March* 12, 1882, *p*. 407.

53.—TRACHEOTOMY—A NEW TRACHEOTOME.

Drs. Hale White and Walter Edmunds have designed a new knife for facilitating the operation of tracheotomy; they call it the tracheotome. One difficulty in the operation is always the introduction of the tube: the operator may pass it in front of the trachea, or miss his opening, and have to make a second, and may lose valuable time, and only succeed after several attempts. To obviate these difficulties, the knife shown in the accompanying cut has been devised. It resembles somewhat a pair of dressing forceps; at that end of the instrument which is to be introduced into the trachea, one of the blades has, at



its lower half-inch, a cutting edge, and is shaped like an ordinary scalpel: the other, narrow and blunt, lies, when the instrument is closed, in contact with the cutting blade, and well behind its edge. At the handle the blades are widely separated, roughened to give the operator a better hold, and connected by a catch and spring, enabling them to be fixed at any degree of approximation. The method of using the instrument is as follows. The trachea having been exposed, either by the cutting edge of this instrument, or an ordinary scalpel, the instrument is held, with the cutting edge upwards towards the patient's chin, and the trachea opened in the usual manner, the instrument is held in position, and its two handles are pressed together, and the two blades thereby separated, thus widening the tracheal opening, the catch holding the handles in the position to which they have been brought. The patient can now breathe easily. The trachea being held widely open by the "tracheotome," the child should be allowed to expel any membrane it can, and the surgeon has ample opportunity of inspecting the back of the trachea, and of removing any loose membrane seen lying there. An ordinary tracheotomy tube can now be introduced with the greatest ease between the two widely-separated blades in the trachea, and they can be withdrawn as soon as the tube is in position. The advantage claimed for this instrument is that the operator never loses his hold on the first opening he makes in the trachea. It is made by Messrs. Millikin and Down.—*British Medical Journal, Dec.* 10, 1882, *p.* 942.

54.—TREATMENT OF GUN-SHOT WOUNDS OF THE ABDOMEN IN RELATION TO MODERN PERITONEAL SURGERY.

By J. Marion Sims, M.D., LL.D., &c.

Peritoneal surgery is a new domain, just opened to the profession at large by a few bold pioneers, who, in science as in the physical world, go before and blaze the way for us to follow and take possession. The principles essential to success, which guide us in all these operations, were neatly formulated by Mr. Spencer Wells at the meeting of the late International Medical Congress. They are:—

1. All hemorrhages must be promptly controlled by pressure, ligature, or hæmostatic forceps. This principle is common to all operations. 2. The peritoneal cavity must be thoroughly cleaned after operation, and before the abdominal incision is closed. This is the great lesson taught by Thomas Keith, and followed by all successful operators. 3. The abdominal incision, usually in the middle line, must be properly closed.

Twenty years ago, Spencer Wells performed some experiments on the lower animals to prove the importance of uniting the divided edges of the peritoneum at the time of uniting the edges of the parietal section; and, as the propriety of this had lately been questioned, he thought it worth while to bring his pathological specimens from the Museum of the College of Surgeons before the late International Medical Congress, to demonstrate anew the great truth, long ago fully proven. But, independently of Spencer Wells's timely philosophic experiments on the lower animals, we have the best reasons, clinically, why we should always reunite the severed edges of the peritoneum. If the edges of the peritoneum are not embraced in the sutures that close the abdominal section, a raw surface is left on the inner surface of the wound, which immediately adheres to the subjacent parts. If it happens to adhere to the omentum, well and good; but if to intestine, the result may or may not be fortunate. For, if the adherent intestine happen to be convoluted in such way as to obstruct the bowel, a fatal result may follow; and we cannot afford to risk the possibility of such accidents. Hence the necessity of uniting the divided edges of the peritoneum. Clinical experience furnishes still another argument why we should always unite the divided edges of the peritoneum. I have seen three cases in which the edges of the peritoneum were firmly united, while the parietal wound gaped widely open. Thus, if the peritoneum had not been closed, there would have been no union whatever in the line of abdominal incision.

There is another principle in peritoneal surgery which is still *sub judice*, and that is—4. Drainage or no drainage. Chassaignac was the first to demonstrate the importance of drainage in general surgery, and no one now pretends to dispute its value. He was the first to point out the source and dangers of septicæmia and pyæmia, and at the same to designate a preventive in his *tubes à drainage*. The precepts and practice of Chassaignac are now accepted and acted upon every day and everywhere, but the name of this great French surgeon seems, for the moment, to be forgotten in this relation. In general surgery, complete drainage is essential to successful results. We cannot now dispense with it, whether we use antiseptics or not. If, then, drainage is so important in general surgery, why should we be so much afraid of it in peritoneal surgery? There is no special danger in introducing a glass drainage-tube into the peritoneal cavity at the lower end of the abdominal incision; for it immediately becomes sacculated, and thus nature protects the peritoneum against the presence of a foreign body in its cavity. If there be no bloody serum to drain off, the tube can be removed in a few hours; but if there be any serum, it soon makes its appearance at the surface, and is readily absorbed by sponges placed to receive it.

The drainage-tube is now wholly excluded by Spencer Wells, Thornton, and many others, on the theory that Listerism renders the peritoneal effusion aseptic, and therefore that its absorption will not be attended with danger. But is this always so? In my early operations I occasionally saw cases where the accidental discharge of bloody serum through the external wound gave prompt relief of urgent symptoms, and led to speedy cure. All other operators have had a like experience. With me, these were before antiseptics and drainage tubes. But even now, under the best antiseptic precautions, are not such cases met with occasionally?

In December 1878 I assisted my friend Mr. Spencer Wells with an ovariotomy in the suburbs of London. The case was a very bad one. Knowing full well its difficulties and dangers, he had wisely procrastinated the operation to the latest moment compatible with safety to his patient. Adhesions in the bottom of the pelvis were universal and very strong, and it was difficult to arrest the exudation of blood. When the external wound was being closed, Mr. Wells saw that there was some exudation still going on; but, thinking that Listerism had rendered it aseptic, he had no fears for the result. The patient did well for about thirty-six hours, when she became rapidly septicæmic, and fears were naturally felt for her safety. Fortunately, just at this juncture, bloody serum was found exuding from the lower angle of the wound. Mr. Wells then removed some of the sutures, and opened the wound a little; there was a free discharge of septic fluid, and the patient made a rapid recovery. Now, I do not pretend to say that this patient would necessarily have died, if nature had not so unerringly pointed out the method of immediate relief to urgent symptoms. She might possibly have safely eliminated all this septic fluid; and then, again, she might not. But of this I am sure: if the drainage-tube had been used at first, the poisonous bloody serum would have been drained off as it was extravasated, and there would not have been the least cause of alarm for the safety of the patient.

The only valid objection that can be urged against the drainage-tube in abdominal surgery is, not in its immediate danger, but in its ultimate tendency to develop ventral hernia. And this is a serious objection, which we, who advocate its use, must learn to obviate. This is a problem to be worked out, and I am sure it can and will be done. But till then it is better, in doubtful cases, to risk the production of ventral hernia, with all its inconveniences, than to risk the life of the patient.

So much for principles of treatment governing all important peritoneal operations. Now let us see what has already been accomplished in this department of surgery in the last ten years, and then we can determine with greater certainty what we may reasonably expect in the next. As my argument depends wholly upon what has already been done with such wondrous success in the domain of peritoneal surgery, I must be allowed to state the premises from which my conclusions are drawn.

Extirpation of the uterus for bleeding fibroids was a legitimate sequence of ovariotomy. At first, it was done by accident, then intentionally. The early operations were not successful; but now, Kœberlé and Péan on the Continent, and Spencer Wells and Thomas Keith in England, can boast of magnificent results; and already it is an accepted operation in properly selected cases. Péan's peculiar method of operating is by removing portions of the tumour, *morcellement* (as he terms it), till it is small enough to be easily turned out of the abdominal cavity. He then makes a pedicle of the cervix, and secures it in the lower angle of the abdominal incision, as we formerly did with the pedicle in ovariotomy.

The late Dr. Wright, of Cincinnati, the most successful operator in our country, tied the broad ligaments separately, amputated the uterus, then scooped out the cervix funnel-shaped, and brought together the opposing surfaces antero-posteriorly, united them by suture, and dropped the stump back into the peritoneal cavity, and closed the wound. Thus the amputated cervix was covered over with peritoneum, which protected the viscera against the dangers of adhesion to a raw cut surface.

Schröder, of Berlin, does the same, not knowing he had been preceded by our countryman, Dr. Wright.

Zwann, of Holland, greatly modifies Péan's method of operation. He makes the abdominal incision large enough to draw the tumour out of the cavity at once. After this he then rapidly introduces three or four temporary sutures, closing the incision sufficiently to prevent the prolapse of the intestines. He next encircles the pedicle with a strong elastic cord, on the principle of Esmarch's bloodless method. He then amputates the tumour just above the cord, and finishes the operation, as Péan does, by transfixing the pedicle antero-posteriorly, securing the ligatures one on each side, and bringing it out at the lower angle of the wound and fixing it there. After this, the abdominal incision is neatly brought together by sutures. The advantages claimed by Zwann are: 1. Facility of operating; 2. Protection of abdominal organs against sudden chill; 3. Prevention of prolapse of intestines; 4. Bloodlessness of operation.

One of the most important advances in peritoneal surgery in connection with bleeding uterine fibroids is Battey's operation to bring about the menopause. It is likely to be substituted entirely for the more formidable operation of extirpation. It is less dangerous; it promptly arrests the bleeding. The fibroid growth begins immediately to decrease, and in some instances it has wholly disappeared.

Freund's operation of extirpating a cancerous uterus by abdomino-vaginal section has not fulfilled the expectations of its author.

Spencer Wells has recently performed successfully a Freund-Porro operation, extirpating a pregnant uterus at the sixth month, in which the cervix was cancerous (Brit. Med. Journal, Oct. 29, 1881). Bantock has also lately extirpated with success a cancerous uterus by Freund's method. These gentlemen have greatly simplified the operation (*ib.*, Nov. 12, 1881).

Half a century ago, or more, Blundell suggested the removal of the cancerous uterus by the vagina, and performed the operation. The Blundell vaginal operation has recently been performed successfully by Prof. Beverly Cole, of San Francisco. He separated the cervix uteri from its attachments with a Paquelin cautery, pulled down the uterus, secured the broad ligaments, and removed the uterus by a comparatively bloodless operation.

Dr. Lane, of San Francisco, late Professor of Surgery in the University of the Pacific, has performed this operation successfully. So has Dr. Clinton Cushing, of San Francisco. Thus we see Blundell's operation for extirpating the uterus through the vagina has so far been monopolised by the surgeons of San Francisco.

Extirpation of the spleen cannot be claimed as an offshoot of ovariotomy. According to statistics worked up by the late Dr. Otis, of the Army Medical Museum, Washington, the spleen has been extirpated between 1549 and 1849, sixteen times with but one death; and, between 1849 and 1869, ten times with five deaths. The deaths were from hemorrhage, immediate or secondary.

Splenotomy has lately been done by Péan, Spencer Wells, Martin (of Berlin), and others. I assisted at Spencer Wells's last operation. The patient died of secondary hemorrhage. The spleen weighed ten pounds, and there were three pounds of blood in the peritoneal cavity. In this operation it is important to tie the pedicle in segments, to guard against the possible slipping of the ligature.

Extirpation of the kidney by the lumbar region has been often done successfully. Martin (of Berlin) has removed the kidney six times by abdominal section, with four recoveries. The operation has been done by others. — *British Medical Journal, Dec.* 10, 1881, *p.* 925.

55.—ON THE RADICAL CURE OF HERNIA.

By W. **Alexander**, M.D., F.R.C.S.Eng., Surgeon to the Liverpool Workhouse Infirmary.

The only operation that is recommended is that which goes under the name of Wood's operation, the long and complicated description of which is generally more tedious than the operation itself. I have performed it in two cases.

Case 1.—James C., æt. 15 years, has suffered from left scrotal hernia for five years. The hernia appeared after a strain, and gradually grew larger, until it became unmanageable by a truss. He then came into the workhouse hospital, where different trusses were tried, but none of them effectually restrained the protrusion of the gut, even during the sedentary life of a patient. On March 9, 1881, I performed Wood's operation, and brought the pillars of the ring, the conjoined tendon, and Poupart's ligament well together in the usual way. The operation was performed antiseptically. The highest temperature experienced was 101·2° on the evening of the third day after operation. The sutures were removed on the 2nd April. When he was about to leave hospital he was attacked with rheumatic fever, which necessitated his stay in hospital for some time. He then went to a convalescent home, and finally disappeared in July. The operation seemed then to be quite effectual, and he was advised to wear a light truss only as a safeguard.

Case 2.—Anthony R., æt. 5 years, was the subject of a large congenital left scrotal hernia. No truss was effectual, and the scrotum was gradually becoming larger and larger. Wood's operation was performed on June 1, 1881. The canal was so much distended that two fingers could easily invaginate the loose skin of the scrotum, and move about in the abdominal cavity. The boundaries of this large opening were approximated in the usual way. The highest temperature, 102·4°, was reached on the evening of the second day. Next morning the thermometer only showed 99°, and the temperature afterwards was normal. The stitches were removed on the 23rd. On 9th July he was sent to a convalescent home, with the hernia apparently cured. I saw him again on 9th August, with the hernia again protruding, and with the pillars of the canal so separated that the operation was a complete failure. The matted tissues that had filled the aperture were now halfway down into the scrotum.

I shall now describe an operation for femoral hernia similar in principle to that of Mr. Wood's for inguinal. According to Mr. Holmes it has only been performed once before, and in that case was fatal by perforation of the intestine. Eliz. N., æt. 30 years, was admitted to hospital on Dec. 21, 1880, with a left reducible femoral hernia the size of a large orange. She was put into the hands of the truss-maker, and after repeated attempts to accommodate her with a well-fitting truss he gave up the task in despair. The patient was quite disabled by the rupture, not only from doing light work, but even from walking about. On 1st June I freed the skin from the subcutaneous structures over an area of two inches around the saphenous opening. I invaginated the subcutaneous tissues and the sac well into the canal, and raised Poupart's ligament on the point of the invaginating finger as close to the femoral vein as possible. I passed a needle, threaded with a silver wire, through the ligament upon my finger, and led the point of the needle across the canal by means of the finger until the edge of Gimbernat's ligament was reached. Through this the needle was passed and the wire drawn through. Another suture was passed in a similar way, internal to the first. Both sutures were tied, and Poupart's ligament and Gimbernat's were then found to be so approximated that the canal was completely occluded. No constitutional symptoms whatever followed, and the wound healed up rapidly. A month afterwards the hernia returned, and the operation failed, as I almost expected it would.

These are the only operations I have performed on the principle of the approximation of the boundaries of the hernial canal. The first case was still successful after the lapse of four months. The other two cases were failures.

Now, even if Wood's operation was effectual in inguinal hernia, it would still be necessary to discover some operation that would be effectual in the other forms of hernia to which neither Wood's operation nor any modification of it is applicable. But there is a strong impression, and in all probability a true impression, that Wood's operation, even in inguinal hernia, is rarely successful, or, if apparently successful, only temporarily so. Hence, many operators for the radical cure of hernia have of late years turned their attention to operations on the sac, rather than to operations on the walls of the hernial canal. Similar operations had been before performed, but owing to their risk had become obsolete, and their revival is no doubt due to the improvements in the treatment of wounds that have been introduced during the last fifteen years. The sweeping condemnation that such operations receive when mentioned in all but the most recent text-books, may therefore be set aside as behind the times.

I dare say most of you have known cases of hernia that were cured by the application of a truss, and by that only. In young children small umbilical hernias are not uncommon during the first year or more of life, that afterwards completely disappear. In such cases a contraction is found to take place at the neck of the sac by which the canal of the neck is completely obliterated, and the cavity of the sac cut off from the abdominal cavity. A firm ring of fibrous tissue surrounds the neck, and it was the contraction of this tissue that produced the occlusion. The sac may remain collapsed and become obliterated, or it may fill with fluid and form a cyst, so that the *natural* cure of a hernia is produced by a *gradual ligature of the neck of the sac at the internal ring*. But this natural cure of hernia is of comparatively rare occurrence, because the fibrous tissue that in such cases produces contraction is also capable of extension under the pressure of the intestines, and it is only under a rare combination of circumstances that a hernial protrusion remains so long and so completely reduced as to allow this contraction to occur. Hence, the radical cure of hernia that I am about to describe of artificial occlusion of the hernial sac at the internal ring.

But it may be asked, Is not a hernia the result of a congenital or acquired weakness of the abdominal wall, and though we may ligature the neck of the sac we do not strengthen the weakness of the surrounding tissues, and will the hernia not recur immediately? To such a question there are two answers, the theoretical and the practical. The theoretical I will give now.

The abdominal cavity is a confined space lined by a smooth serous membrane, and, practically speaking, filled with intestines that flow about almost like a fluid, and that press equally upon all parts of the internal surface of the abdominal walls. This flowing quality of the intestines is familiar to all who have had much experience in abdominal surgery. Whilst the lining membrane of the abdomen continues flush all over, there can be no inequality of pressure, but should a congenital chink or *cul-de-sac* exist, or should a temporary excessive tension be so directed as to accidentally cause a bulging out of the peritoneum, then the normal conditions become at once altered. The tension on that spot becomes greater on account of the greater area of its surface, and as the pressure becomes greater so does the surface. Surface and pressure increase *pari passu*, and mutually assist each other. The abdominal supports are separated from each other, and the abdominal wall at that spot becomes very weak indeed. If we now ligature the neck of the sac and make the peritoneum once more level across the aperture, we at once take away the abnormal pressure of the intestines by limiting the area of pressure to the normal amount; the abdominal supports that were pushed aside will return to their normal position since there is nothing to prevent their tendency to return; and the state of affairs will be much the same as if a hernia had never existed there. Mr. Wood's operation succeeds where the canal is, by the operation, completely occluded up to the internal ring. But this rarely happens in that operation, and the small *cul-de-sac* that is generally left becomes a focus for tension that soon causes the cicatricial tissue to give way and the hernia to recur.

The practical answer will be given afterwards by a narration and exhibition of cases. The essential part of the operation consists *in ligaturing the neck of the sac close up to the internal ring so as to make the peritoneum flush over the inner opening.* Unless this is done effectually the operation will fail. I make a linear incision over the neck of the sac, and in a line with the direction of the hernial protrusion, cut through the coverings of the hernia until the sac is reached, reduce the hernia, and open the sac sufficiently to allow a purified finger to be inserted into the neck. The finger is used to certify that the sac is clear of intestine and omentum, and to indicate the position of the ligature. The neck of the sac only is now freed from the surrounding tissue by a director or probe, and a double catgut ligature passed round it by means of an aneurism needle. If the hernia is an inguinal one in a male, the cord is separated from the sac and excluded from the contents of the ligature. The ligature is now tightened as the finger is withdrawn, and knotted in the usual way. I then cut through the neck of the sac below the ligature, and close up the wound, but not so completely as to prevent free exit for any exudation. The operation is performed with all the precautions of the spray-gauze method. Such is what I consider the typical operation for hernia, *by ligaturing the sac.* The excised sac has vital connections with the surrounding tissues, and does not slough, and the cutting through of the sac produces a firmer union than would occur were the sac only ligatured. But the sac may be completely extirpated, or its neck may be only ligatured and otherwise not interfered with; or the sac may be raised from its bed and allowed to slough, and each of these modifications may have their suitable cases.

I shall now narrate briefly two cases of femoral and two of inguinal hernia where the operation of ligature of the neck of the sac was performed. My first case of inguinal hernia was the little boy Anthony R., æt. 5 years, upon whom Wood's operation had been performed and had failed. The operation was performed on 10th August, and, with a little more difficulty than usual, on account of the thickened tissues left from the former operation. The operation was performed in what I have described as the typical way. An ice-bag was laid over the antiseptic dressing during the afternoon. The evening temperature was 97·4°. Next morning the temperature was 99·2°, and the same on the evening of the second day. It was afterwards normal. There were no constitutional symptoms. The bowels were moved for the first time on 19th August, and the wound was quite healed on 24th August.

My second case was John R., æt. 13, the subject of a large reducible congenital left scrotal hernia—no truss was efficient; and his teacher told me that the rupture was the source of great distress to the boy. The same operation was performed as on the last case, an ice-bag was applied. His temperature rose to 100° on the second day, and was accompanied with some orchitis. This rapidly subsided, and the wound healed in a manner that leaves no room for remark. Since the operation neither of these patients have worn any truss or hernial support.

The chief recommendation of this cure of hernia is that it is applicable to all kinds of hernia, and I will now relate briefly two cases of femoral hernia upon which I operated some time ago. The first case was Elizabeth N., æt. 30 years, upon whom I had performed the rare but ineffectual operation of approximating Poupart's to Gimbernat's ligament previously described. This was really my first operation for ligature of the sac, and in this case I raised the entire sac from its surroundings, ligatured its neck, and then partially closed the wound. The sac gradually sloughed, and the healing of the wound was proportionally prolonged. As she had been a great sufferer from the rupture, and as I was specially anxious to relieve the patient rather than to test the permanence of the cure, I advised her to wear a truss as a safeguard. I saw her a fortnight ago in the street. She told me she had continued quite well, and was still wearing the truss. I have very little doubt she is continuing to be relieved by the operation, or she would have returned to consult me.

My second case was Ellen W., æt. 38, the subject of left femoral hernia for several years. She was in the habit of wearing a truss, but it was either inefficient or carelessly worn, as strangulation apparently occurred some time ago, and was relieved spontaneously after two days' vomiting. On 21st October 1881 vomiting began again, accompanied by pain and tension in the rupture. As she thought it would pass off she did not apply for relief till 30th October, when she came into hospital with all the symptoms of strangulated hernia. I performed herniotomy antiseptically, and opened the sac, where I found a considerable quantity of dark sanguinolent flaky serum, and a strangulated loop of dark intestine extensively coated with lymph. The sac and intestine were cleaned with warm solution of carbolic acid, the obstruction cut, and the hernia reduced. I now pulled the neck of the sac well down and passed an aneurism needle round it, and tied it with thick catgut. Temperature during that evening and next day was successively 99·4°, 100°, and 101°. Next morning it was 99·4°, and was afterwards normal. There was no sickness or constitutional symptoms except thirst. Morphia and atropine were administered every evening for three days. The bowels were opened on 6th November, and on 7th November she ate a chop. The wound has now healed without any sloughing, but I have not brought her round, as it would be unsafe to strain the tissues just yet.

In umbilical hernia I have seen a radical cure performed by a general practitioner many times. The coverings of the hernia are very meagre, and the way in which the cure was performed was to reduce the protrusion and ligature the base of the sac outside the skin. The ligatured portion dropped off. Yesterday I performed the radical cure of umbilical hernia by making a small opening at one side of the large protrusion through the skin down to the sac. Through this opening, by means of a probe, I passed a catgut ligature subcutaneously round the neck, and pulled it tight after reducing the hernia. The operation was performed antiseptically, and dressed with an immovable gauze dressing which will not require any change until long after the small wound has healed. By that time the parts will be strong enough to resist the internal pressure.

Of the other methods of dealing with the sac, there is Mr. Warren's method of producing inflammatory adhesion round the neck of the sac by the subcutaneous injection into the tissues around it of a preparation of white oak bark. Mr. Buchanan dissects the sac and rolls it up into the opening. Mr. Annandale ties the neck of the sac and cuts off all below. Wutzer inverts the sac and retains it in its place by a well-known apparatus, and Mr. Spanton obliterates the sac by a spiral metallic coil. Wutzer's method is, I think, now quite disused. The other methods are still *sub judice*, and of them I have no practical experience.—*Liverpool Med.-Chir. Journal, Jan.* 1882, *p.* 124.

56.—OPERATION FOR THE RADICAL CURE OF HERNIA.

By W. D. Spanton, M.R.C.S., Surgeon to the North Staffordshire Infirmary.

What operation is best adapted to the purpose? The older ones are undoubtedly unsafe and unjustifiable, and I cannot see how most of them could effect the desired result. And, unhappily, the want of success in the past, and the high rate of mortality attending even so recent an operation as Wutzer's, have deterred all cautious surgeons from advising or practising them. But with the experience of the past to guide us, and the resources of modern surgery to help us, success must come sooner or later—and why not now? One thing is quite clear, that if we never try we shall never succeed; and to give an operation fair trial, it must be carried out on a large scale. We cannot hope to have no mortality at all, but if it is so low as to approach that which is considered legitimate for other daily operations of expediency then I take it we are more than justified in adopting it.

I have a record of nearly forty cases of inguinal hernia in which the operation I have already published an account of has been performed, and although I am unable to give the exact number actually cured, it is, I believe, much more than half; and there has not been in any one case any really dangerous symptoms. Patients are often classed as "cured" on leaving hospital, but I mean by cured those in whom the cure is so complete that no truss is required. In fact, a cure can hardly be said to be effected so long as a truss is looked upon as essential to be worn. Some operators lay so much stress on the necessity for a truss to be worn long afterwards, and exact so many extreme precautions, that some distrust is naturally excited in one's mind as to the thorough efficiency of their operation.

I have brought with me two patients on whom I have operated, one nearly two years ago and the other about one year since; and the mode of procedure in the case of the elder one was this:—After the usual preparation in such cases, ether being given, standing on the left of the patient, an incision was made in the skin of the scrotum large enough to admit one's finger, downwards and outwards, and the skin separated for about two square inches in extent. The hernia having been previously reduced, the left forefinger was passed into the scrotal wound, pushing before it sac and scrotal fascia high up into the inguinal canal, so that the finger could easily make out the condition of the abdominal rings and surrounding structures. The invaginated tissues being so held by the finger, the point of the screw was, with the right hand, thrust through the skin of the groin so as to pass through the conjoined tendon at the internal ring in such a way that the point came against the tip of the left finger. The screw was next made to transfix the sac and fascia held in the inguinal canal, and so to pass across to the external pillar subcutaneously. A turn was then given to the screw, so as to push it through the internal pillar of the external ring and again across to Poupart's ligament, the point emerging through the scrotal fascia at the wound. In this manner the sides of the hernial canal are approximated throughout, and the plug of invaginated tissue is firmly held in position while consolidation is taking place. At the same time the general cavity of the peritoneum is securely shut off. The left finger protects the spermatic cord throughout, and is withdrawn as the screw traverses downwards, the point of which should be felt by the finger from first to last. The handle of the screw then lay flat on the abdomen, and the point was protected by a ball of India-rubber, and the scrotal wound closed by a suture of catgut. There was no hemorrhage worth mentioning. Carbolic spray and Lister's dressings were used. The screw was removed on the 9th day, and the dressings relinquished on the 15th day. He was allowed to get up on the 28th day, the parts being firmly consolidated, and no impulse was felt on coughing. Two days afterwards he returned home.

I have found in my cases as a rule that the best results have been obtained where suppurative inflammation has been less rather than in those where it has been more free; but much would seem to depend on the individual. We find some persons more prone to adhesive inflammatory action, others to suppuration; and it is among the former we obtain the most successful hernial cures. It is, I believe, for this reason we have found the screw to answer better than a ligature, which was employed in some of my cases; and Mr. John Wood, probably for the same reason, found it desirable to discard silk or hemp, which he used in his earlier cases, in favour of wire. It is a mistake, I think, to retain the screw in too long. About a week or eight days is usually long enough for it to become loose, so that it can be readily removed. I advise a pad to be worn for some time, not as an ordinary truss, which promotes absorption of the newly thickened tissues, but merely as a support under any undue exertion, such as coughing, &c., before the parts have become thoroughly consolidated. The advantages which seem to me to belong to this operation are the effectual plugging of the whole of the hernial canal, the secure retention of the plug by the screw during the healing process, the small amount of local and constitutional disturbance, and the ease and safety of the performance of the operation itself. The pain occasioned is far less than might be anticipated, and the time occupied is usually of little importance to those for whom it is best adapted. Assuming an operation of some kind to be both justifiable and necessary, I should like to say a word as to the class of cases in which it is required and most likely to be successful.

The most suitable are those occurring in the young, *i.e.*, during childhood and adolescence. These succeed best, and are just those in which it is most desirable to effect a complete cure. There need be very few if any cases of congenital hernia in adults, and if these alone were rendered sound during early life, we should at once get rid of about one-eighth of the whole number of hernia cases in this country. The next most fit cases are those in active young men, in whom accidental rupture has taken place, and who are thereby unfitted for many social duties; and from their inability to enter any of the public services, are a direct loss to the State. In the same way some young women are proper subjects; as in one instance in which I operated, because the patient wished to be married, and did not like to do so with a hernia existing. I am happy to say that in this case the young lady lost her hernia and obtained a husband within a very short period, much to her gratification in every way. Each case must be decided on its own merits. None of us would, I feel sure, desire to advocate indiscriminate operating by any means. Let the suitable cases be carefully selected, and we shall find them sufficiently numerous eventually to overcome the prejudice which at present exists both in the public and the professional mind against any operation whatever.—*Liverpool Med.-Chir. Journal, Jan.* 1882, *p.* 136.

57.—ON THE SURGICAL TREATMENT OF HEMORRHOIDS.

By Walter Whitehead, F.R.C.S.Ed., F.R.S.Edin., Surgeon to the Manchester Royal Infirmary.

The intention of the operation is to remove from the lower segment of the rectum the diseased, dilated, and tortuous vessels, and the adjacent tissues, hypertrophied and consolidated by plastic exudation. These diseased structures are situated beneath the mucous membrane of the rectum, and rest upon the circular fibres of the internal sphincter; and it is the object of the operation to clear them away from the anal orifice, where they act as obstructions to defecation, and excitants of periodic and distressing spasmodic muscular contractions.

During the last five years, I have excised hemorrhoidal tumours on a plan which I believe to be in harmony with the principles of modern surgery, and have abandoned those pyrotechnic aids and cumbersome appliances which at one time I regarded as indispensable. The following cases, selected out of a large number, will perhaps explain my manner of conducting the operation, and the principles upon which it is founded, better than a more formal description.

In June 1880, I saw, at the request of Dr. Leech, a lady of robust figure who had suffered for nine years from internal hemorrhoids. She had been advised seven years previously to have an operation for their removal, but feelings of delicacy had interfered with her consenting to follow the advice. Her life had become a burden, in consequence of the absorbing demands imposed by the hemorrhoids. Her nervous system was demoralised by the concentration of her thoughts on her trouble; and the habits of her life had to be regulated by the exigencies of her ailment, until finally she felt compelled to submit to any plan of treatment that could afford her a prospect of relief. After it had been decided to excise the hemorrhoids, a day in the following week was fixed for the operation, and in the meantime the patient was very carefully prepared by diet, aperients, and rest. The operation was conducted under chloroform, with the patient in the lithotomy position. As a preliminary measure, the functions of the sphincter were suspended by forcible dilatation. Two thumbs were introduced into the rectum, and the circumference steadily kneaded in every direction until all resistance was overcome, and the sphincter rendered absolutely passive. The patulous condition of the rectum thus obtained enabled the whole mass of piles to be protruded through the anus, by introducing two fingers into the vagina and depressing the recto-vaginal wall. The hemorrhoids in size and appearance resembled an average ripe tomato, and were mapped on the surface into four irregular and unequal lobes. The lobes were next divided into four segments by longitudinal sections in the axis of the bowel, and in the furrows marking the intervals between the several lobes. This was accomplished without the loss of any blood. Each portion was then secured in succession by ring-forceps, and dissected with scissors; first, transversely from the anal margin, and then the dissection was continued upwards in the cellular plane to the highest limit of the hemorrhoidal growths, in this case about an inch and a half. Each segment was thus converted into a quadrilateral wedge-shaped mass, the base below consisting of the hemorrhoids, and the apex above of the healthy mucous membrane of the bowel. The mucous membrane at the highest point was next transversely divided, leaving the hemorrhoids simply attached by loose cellular tissue, and the vessels proceeding from above and supplying the mass below. The forceps containing the hemorrhoids were then twisted until a connection was severed and the hemorrhoids removed. The divided surface of mucous membrane was next drawn down, and attached by several fine silk sutures to the denuded border at the verge of the anus. The other portions having been treated in the same manner, the operation was completed. The sections throughout were made by scissors. The loss of blood during the operation did not exceed a couple of ounces. The patient made a complete recovery, and regained the full capacity to discharge her domestic duties and social engagements.

Another case was operated upon with Mr. Sutcliffe, of Stretford Road. The patient was a young lady, twenty-five years of age, whose health and strength had been alarmingly reduced through nine years' pain, mental distress, and hemorrhage occasioned by internal hemorrhoids. Such, indeed, was her anæmic condition, that, had I felt less confidence in the small amount of blood likely to be lost, I certainly should have hesitated before undertaking the operation. The piles were unusually large and exceedingly vascular, jets of arterial blood projecting from minute orifices in the tumour upon the application of the slightest pressure. The operation was conducted on the same lines as the previous case, and with equally satisfactory results. The loss of blood during the operation was so slight, that only one sponge was used. Immediately after the patient was returned to her bed, a two-grain belladonna suppository was introduced into the rectum, and a quarter of a grain of morphia administered subcutaneously. The patient suffered no pain whatever after the operation, and her convalescence was complete in a couple of weeks, and remained permanent.

Another example affording considerable interest, and associated with features of unusual character, occurred in the case of a married lady on whom I operated in the summer of 1879. The hemorrhoids would appear to have originated from the pressure exercised by a retroverted uterus. The position of the uterus not only interfered with the circulation in the rectal vessels, but also, by impinging on the sacrum, acted as a kind of valve and intercepted the descent of fæces. The use of aperients only increased her difficulties, by causing increased straining and a firmer impaction of the uterus. She also suffered from excessive and occasionally alarming loss of blood. Ten years of suffering, with intervals of relief during the latter stages of three pregnancies, were endured by this patient before she would submit to any operation. In order to convey an idea of the ignominy to which humanity may be reduced by hemorrhoids, I may mention that this lady, who possessed feelings of the greatest refinement, admitted that during the last five years she had been forced by experience to adopt the revolting expedient of relieving her bowels by the use of her fingers. The retroversion was corrected by a suitable pessary, and the hemorrhoids were operated upon in the same manner as the other cases. An attack of cystitis supervened, and complicated what would have otherwise been an excellent recovery. The bladder-symptoms, however, speedily responded to simple remedies, and the patient left town for her home in fourteen days after the operation. Since that time there has been a relapse of the uterine troubles, but, so far a the hemorrhoids are concerned, the cure has been entirely successful.

These three cases may be taken as representing a large number on which I have operated, in hospital and in private practice, during the last five years, and all have been followed by uninterrupted recoveries and admirable results; no cases have been complicated by secondary hemorrhage or any constitutional ill effects. The amount of pain following the operations has not been uniform. In some cases there has been an absolute freedom, whereas in others pain has been complained of during the first few hours after the operation. The number of instances and the amount of pain, has, however, gradually diminished as experience has been gained in the manner of conducting the operation and in the treatment subsequently adopted.

There are points in connection with the operation which will perhaps admit of further explanation. The operation is based in the first place upon the anatomical foundation, long since demonstrated by Quain, that the arteries in the lower part of the rectum descend from above, running vertically in parallel lines towards the end of the gut in the cellular tissue between the mucous membrane and the muscles. It is from this arrangement of the vessels that the surgeon is enabled to leave the torsion of the arteries to such a late stage of the operation.

The dominant influence of the sphincter, as a factor in the causation of hemorrhoids, is found equally potent as an agent during the after-treatment; and unless its influence be totally compromised by forcible distension, secondary hemorrhage may be concealed within the rectum, and much subsequent suffering endured from spasmodic contractions. With the sphincter dilated, however, secondary hemorrhage ceases to be a consideration of importance; and if during the operation collateral arteries be divided and bleed freely, they are treated on ordinary principles and twisted without difficulty. In the healthy rectum, the mucous membrane is loosely connected with the adjacent muscle and readily detached, but in this operation it is one of the objects, and a main feature in the cure, to obtain adhesion between the mucous membrane and the muscular coat of the bowel, in order to counteract for the future the tendency to hemorrhoidal stasis by giving a substantial support to the vessels; and this is gained by uniting the healthy mucous membrane from above to the verge of the anus—an advantage which cannot be overestimated. It closes what would otherwise be an open wound in one of the most undesirable localities of the body, and, by protecting the raw surface from the irritating influences of passing fæces, prevents a considerable amount of after-suffering, and admits the only possible chance of immediate repair.

A contingency that will at once suggest itself to the minds of those who read this description is the risk of stricture likely to follow the cicatrix resulting from this plan of operation. I may mention that, wherever it is feasible, with strict regard to removing every evidence of any hemorrhoidal growth, I invariably leave longitudinal strips of mucous membrane continuous with the skin; but in severe cases, requiring the removal of the entire circumference, I have no fear of the bowel being inconveniently contracted when mucous membrane alone is sacrificed; and believe that undue contractions only take place when annular cicatrix is formed at the expense of integument. I have taken great pains to ascertain that this fear is groundless, and I have watched most of my cases for a sufficient length of time to relieve my mind from any further anxiety on this point; at the same time, I fully realise that the progress of such contractions is slow.

The preparation of the patient, previously to operation, requires a considerable amount of forethought and personal supervision. The patient must be induced to remain recumbent for at least three days previously to operation. An immense gain is secured by this—the hemorrhoids are reduced to their least possible dimensions; whereas, if the patient be allowed to go about as usual, he frequently exerts himself in making unusual arrangements in anticipation of the operation, and by such means increases the vascularity of the hemorrhoids, and consequently adds unnecessarily to the difficulty and the extent of the operation. It would be obviously unscientific to operate upon piles during an "acute attack;" and it is equally evident that, the more the circulation can be reduced to a quiescent condition, the less will be the bulk of tissue requiring removal.

The diet must also be regulated by strict rules. In ordinary cases fluid farinaceous food should alone be taken, and, unless specially indicated, all stimulants should be interdicted. Suitable aperients must be prescribed, and the character of the evacuations should be inspected, in order to ascertain that no solid fæces remain, and to secure an empty rectum on the day of operation. An enema of water containing a little glycerine may with advantage be administered each morning. A digital examination of the rectum the day preceding the operation may prevent the unpleasant discovery at the last moment of an obstinate accumulation in the rectum, notwithstanding these precautions.

The lithotomy position for the patient during the operation, though perhaps the least delicate, is for the same reason the most convenient, as it permits what is most desirable—an unincumbered view of the parts to be dealt with; and further, it secures, when the hips are elevated and the thighs well flexed on the abdomen, a relaxation of the muscles likely to interfere with free manipulation. I have always found an ordinary dressing-table convenient for the patient to lie upon during the operation, and I prefer sitting on a low chair in front of the patient.

When the patient is in position, I generally commence by compressing a soft sponge about the size of an orange, and passing it six inches or more up the rectum; this precaution prevents the escape of liquid fæces during the operation; and it is hardly necessary to state that the sponge must not be forgotten at the completion of the operation. During the operation, sponges wrung out of spirit and water, one in six, will be found superior to water alone for purposes of clearing blood from the surface of the wound.

In operating on men, the hemorrhoids must be hooked down with a finger, or secured by ring forceps and withdrawn. Attempts to obtain a prolapse of the hemorrhoid by the efforts of the patient, or the use of an enema previously to the administration of an anæsthetic, are undesirable and opposed to sound principles.

The advantages of the operation may be briefly summarised, by stating that the immediate and ultimate results are in every respect satisfactory; and my opinion with regard to the operation may be considered of more moment, when I state that I have had perhaps unusual opportunities of forming an unprejudiced opinion as to the comparative merits of the numerous operations that have been advocated for the removal of hemorrhoids, and I have, I believe, given them each in turn an adequate and impartial trial.—*Brit. Med. Jour. Feb.* 4, 1882, *p.* 148.

58.—TREATMENT OF HEMORRHOIDS BY "CRUSHING."

By R. Fitzroy Benham, M.R.C.S., Baron's Court, S.W.

Since the introduction of my clamp for the treatment of hemorrhoids by crushing, as described by Mr. Pollock (*vide Retrospect*, vol. 82, p. 160), I have had considerable practical experience in this mode of treatment, and propose to review the etiology of the crushing process for the benefit of those who may not be acquainted with the superiority of this method of operating over all others.

Mr. Pollock, some two or three years prior to the communication to which I have just referred, put his views in practice with the object of avoiding the then existing "mode of treatment by clamp, scissors, and actual cautery, which, though perhaps somewhat less painful than by ligature, was still sufficiently severe to occasion much subsequent suffering, and lasting for some days." Besides, the application of the heated iron to the surface of a fresh wound must not only make it a very painful one, but burning the surface necessarily converts it into a more extensive slough, which has to be thrown off before healing commences. It occurred to him "that any thorough and instantaneous destruction of a part is usually comparatively painless in the injury itself and its sequence. A heavy weight falling on a finger or toe, and completely crushing the part, is seldom attended by much pain then or subsequently; nor under such circumstances does much bleeding occur. A man hit by a bullet in soft tissues is known to be ignorant of the receipt of the injury until the trickle of the blood is observed in the neighbourhood of the wound, and thus it occurred to him that if a pile could be rapidly and effectually destroyed at its base by some instrument which in its action would be analogous to that of crushing the part included in its bite, the vessels of the crushed portion would not be very likely to bleed when the surface of the pile was removed, and the nerves being bruised by this proceeding would be less liable to be followed by pain."

Several instruments were designed by others to carry out Mr. Pollock's theory, but he considered that they all failed, namely, in not having sufficient crushing power. These clamps, without exception, were made to apply in a line with, or a parallel direction to, the buttocks. Now it occurred to me that if the pile were to be grasped in the opposite direction, the jaws of the instrument could be so constructed as to be parallel to each other, and on this principle effectual clamps could be made after various designs; but I felt that in order to obtain a clamp of the simplest and yet most powerful description, it would be advisable to construct it in the shape of pincers with handles about four times the length of the other extremity, with the addition of a screw, to be attached to the extremities of the handles. I also thought that instead of the opposite (or opposing) surfaces of the jaws being made square, they should be inclined at an angle of 45°; for this would not only augment the crushing power, but would create a "grinding" movement. A few instruments having been made on this principle, I soon discovered that, although the power which could be brought to bear was almost unlimited, yet for want of an accurate and uniform temperature of metal—namely, a "plum colour," the opposing surfaces of the jaws were apt to become displaced; hence I varied the inclination of the surfaces of the jaws by having them made square, and the crushing area increased, not by thickening the depths, but by introducing a V-shaped piece between them.

I now propose to describe the method of operation adopted by me, and which, I may add, has invariably been crowned with marked success. Prior to the operation, the patient's general health should be thoroughly scrutinised. The bowels should be well opened by the aid of some purgative—perhaps the simplest is either Glauber's salts or the compound senna mixture—and, in addition to this, it is usually advisable to give a common enema just prior to the time fixed for the operation, and to steam the parts in the usual manner. The patient having been placed under the influence of ether, should be turned on one side, usually the left, with the buttocks well drawn to the edge of the bed, and with the knee remote from the bed, well drawn up towards the abdomen. The pile to be removed is to be drawn down by means of a pronged fork or vulsellum. The clamp is then to be applied to the base of the pile, and at once tightly and firmly closed by the action of the screw at the end of the handles. The portion of the pile which protrudes inside the lips of the clamp is then to be removed by a pair of scissors curved on the flat or scalpel. After the clamp has been allowed to remain *in situ* for about a minute, the thumbscrew may be gradually slackened; but, at the same time, particular care must be taken to press the jaws of the instrument well up against the buttocks, so that the surrounding tissues may not be unduly stretched, otherwise they will return into the bowel with an "elastic recoil," and will thus have a tendency to tear asunder the "fringed remnant" left by the clamp. I cannot too strongly impress this on the attention of the operator; for the success of the operation being rendered bloodless depends in no small measure on his taking this precaution. The process is, of course, to be repeated according to the number of masses to be got rid of. If several piles are to be removed it is advisable to operate first on the lower, or those situated more externally, and then to proceed upwards; for by adopting this method the "fringed remnants" left by the clamp from the former masses will not be subjected to so much tension when the latter are operated on. If a pile be of an unusually large size, so much so that it is apprehended that a portion may overhang the sides of the jaws, I clamp the mass in two portions; but before releasing the grip of the jaws from the first portion I divide only a part of it which is within the grip of the jaws, and then take particular care to have a firm hold with the vulsellum of the portion not yet operated on. Occasionally it may be advisable to separate the skin before clamping the pile; but, as a rule, I have not found this at all necessary, or even beneficial, for this reason alone—the area of the wound to be healed is materially increased. After the operation I simply sponge the parts, and usually administer an anodyne, not so much for the purpose of alleviating pain, but chiefly to quiet the patient, as he naturally becomes restless for a few hours afterwards, until the effects of the ether pass off. It is as well that the bowels should not act for about five or six days after the operation, but should they be relaxed I administer opium, either the tincture or a suppository, which I also do in those exceptional cases in which patients are peculiarly sensitive to even the slightest pain. On the morning of the sixth day I usually give about three drachms of Glauber's salts in warm water, and if this does not readily act, which may cause the patient some discomfort, I then administer an enema. By this means the motions become loose, thus reducing the pain arising from the hardened fæces passing along the tender parts to a minimum. After the bowels have acted, the parts are to be carefully sponged and smeared over with carbolised vaseline (five grains to the ounce), by gently introducing the finger into the bowel. The diet should at first be light and nourishing, but not stimulating, unless otherwise indicated. It is rarely necessary for the patient to remain in bed after six or seven days, if the bowels have acted, but it is not advisable to allow him to resume any active work for at least a fortnight after the operation, and, indeed, even for a few days longer, if he be a labouring man.

Having now had exceptional facilities for observing a very large number of patients, besides perusing the notes of innumerable cases extending over very many years, I really cannot speak in too glowing terms with regard to the pain and early convalescence of patients operated on with my clamp, and subjected to the above treatment, as compared with all other methods of operating. In a word, I may say that the difference as regards these respects is truly astounding, so much so that I venture to think that no surgeon acquainted with the fact would wish to submit his patients to any of the what I may now call old methods of treatment. As a rule, beyond a very slight feeling of discomfort or pain for a short time after the operation, which patients have described as being of a tingling and numbed nature, and which may sometimes be experienced when the bowels first act, when, by-the-by, a little blood may sometimes ooze from the parts, I have never heard of any treatment being required, though it is as well to assure the patient that this is quite consistent with a successful operation. With regard to the oozing of blood, if any, the quantity varies; for instance, if the bowels be loose only a few spots may appear, while, on the other hand, if the motions be hard the oozing may be a little freer. Again, I have scarcely ever perceived any constitutional disturbance; and beyond the temperature rising about 1° on the night of the operation it is nearly always normal, provided, of course, the bowels be relieved during the first week. Another most conclusive item in favour of this method of treatment is the fact that I have never found it necessary to use a catheter to relieve the bladder, nor have I ever known of anyone else finding it necessary to do so; whereas, with other methods of operating, the non-use of the catheter is quite exceptional.

I have now performed 19 operations on patients, have acted as the assistant in 83 cases, and have been a busy spectator in 29, in all 131, besides having been in communication with gentlemen who have operated on 51 patients. Of the 131 cases, in not one instance, with the exception of the first two (see Lancet, July 24th, 1880), was it necessary to apply even a fine ligature, so effectual was the crushing process; hence I am in a position to state that, except in the first two cases, when the experiment with the new instrument was necessarily tentative, "I have never *seen* a ligature applied to any bleeding point!" Some of the operations were performed on patients at very critical times; for instance, I operated for a medical gentleman on a lady who was six months pregnant; another patient was operated on most successfully by a surgeon, who had, only a little more than a week previously, performed on her the operation of ovariotomy. With regard to the 51 cases, particulars of which have been forwarded to me, I am aware that in some seven or eight cases, one or more fine ligatures had to be applied to bleeding points, but then it must be remembered that until some surgeons have operated two or three times with an instrument there are likely to be a few slight mishaps. All the other cases were most successful; indeed, it affords me much pleasure to mention that I am informed that a surgeon of world-wide repute has recently operated on his son with my clamp, so satisfied is he of its superiority over all others; and I could multiply instances attesting the fact.

The clamp I use, and which I recommend, is manufactured by Messrs. C. Wright and Co., of New Bond Street, London.—*Lancet, April* 15, 1882, *p.* 602.

ORGANS OF URINE AND GENERATION.

59.—LITHOTRITY AT A SINGLE SITTING.

By Sir Henry Thompson, Bart., F.R.C.S., &c.

The first efforts to crush a stone were made early in the present century, and culminated in the first performance of the operation by Civiale in 1824. After a considerable interval, during which the operation underwent many modifications in the hands of several French surgeons, and not necessary now to be described, a proposal was made by Heurteloup to remove débris on a large scale and accomplish the task at a single operation; but in this he did not succeed. Before this Sir P. Crampton, of Dublin, had invented an apparatus, consisting of a glass receiver, like a large soda-water bottle, from which, by means of an exhausting syringe, he removed the air, and then applied it to a large silver evacuating catheter previously introduced into the bladder. Into this cavity some twelve or fourteen ounces of water had been injected, so that on turning a tap attached to the receiver a powerful rush into it of water and débris took place. This was the first "aspirator." (Fig 1.) Sir Philip was good enough to present me in the year 1854 with a specimen of the instrument, and I tried it, but found it dangerously rough in action, and requiring to be so repeatedly charged and applied that I dared not use it. Soon afterwards Mr. Clover designed an india-rubber aspirator with a glass cylinder, trapped to prevent reflux of fragments, which answered its purpose exceedingly well. The evacuating catheters were silver, Nos. 12 and 13 in size. (Fig. 2.) This apparatus was much used by myself to supplement the action of the bladder, on which action, together with washing out by a forcible current from the eight-ounce syringe, most surgeons (Skey and Coulson particularly) had relied for removing some of the débris made at each sitting. Meanwhile Civiale, who was about this time (1845-65) the most experienced operator in Europe, strongly enjoined the principle of trusting mainly to the natural expulsive power of the bladder, and, shunning anæsthesia, abjured the use of any other instruments except those for thoroughly crushing, as injurious to the organs involved. The same practice was pursued here by Sir B. Brodie, who had a considerable experience, and whose well-known care and judgment conduced to the success of the method. But this over-cautious procedure, as

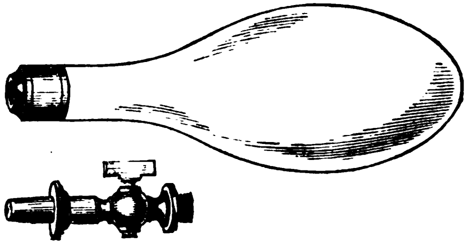


FIG. 1.—Sir Philip Crampton's aspirator.

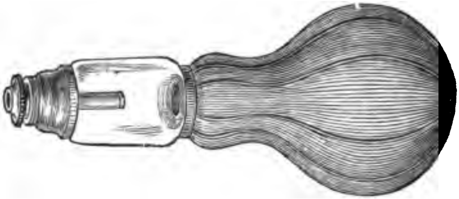


FIG. 2.—Clover's aspirator.

it has turned out to be, doubtless retarded considerably the improvement of the crushing operation. During a somewhat later period, however, Sir William Fergusson was endeavouring to shorten the procedure by removing much débris by the lithotrite itself, introducing it many times during a long sitting under ether; and he succeeded thus in emptying a bladder of a medium-sized calculus at one operation—not, however, without sometimes inflicting serious injury on the neck of the bladder and urethra, by withdrawing large and rough fragments. Lastly, I must add that long before 1870 I was freely using Clover's apparatus for removing débris, subsequently advocating and employing larger crushings, and evacuating by means of it all the débris, however much might remain, when the patient had severe cystitis due to the presence of large or sharp fragments in the bladder. This system I strongly enjoined both here and abroad, where especially the waiting and soothing treatment has been pursued in these circumstances, and application of the lithotrite postponed, in obedience to the powerful traditions of Civiale's teaching.

In 1878 an important change was proposed, as is well known, by Professor Bigelow. He advised that the stone, however large, and without respect to the presence of cystitis or other considerations, should be invariably removed at one sitting, by means of more powerful lithotrites, larger evacuating catheters, and a stronger india-rubber bottle than had before been used. The condition necessary and preliminary to this proposal had been the adoption of a doctrine taught by Otis, of New York, that the ordinary male urethra might be treated as capable of fairly admitting instruments of 16 or 18 gauge, English, or Nos. 20, 30, French; instead of Nos. 12, 14, English, 22, 25, French, which had been generally regarded in the two countries as about the limits of safety in dealing with an ordinary healthy urethra. Once satisfied that the canal would safely admit the larger instruments under the influence of ether, there was no longer reason why a large and hard stone should not be removed, so far as the mechanical procedure is concerned, at one operation. Bigelow believed that it would be safer to empty the bladder, even at the cost of manipulations much more prolonged and severe than had been deemed practicable, than to operate by several sittings with fragments left in the bladder during the intervals of operating. This method has now been employed by several operators, both English and American, but at present has not been generally accepted on the Continent. I tested it without delay in my own practice at the outset, and my own views of its capability may be gathered from the fact that I have employed it largely ever since. Moreover, as early as the autumn of 1880 I had the opportunity of demonstrating the procedure, both in Paris and Vienna, by operating on calculous patients in the public hospitals there. My own experience of it, which is therefore considerable, amounts now to 101 cases in the elderly adult, which have occurred in 101 different individuals, and I do not hesitate to regard it as a valuable improvement on any method previously employed. Further, this experience enables me to place on record not only the results in relation to the all-important question of mortality, but also in some degree to the condition of the patients subsequent to operation, a question which is only second to the former in importance. But I will first describe the various steps of the operation, and show you the instruments which I think the best with which to perform it, and with which I have attained the results referred to. And here I shall revert to the axiom I laid down at my opening lecture, that in the treatment of all diseases of the urinary passages, first, your instruments should be as simple in construction as possible; secondly, that they should be used as sparingly, and always as gently, as the necessities of each case admit of. By lightness and suppleness of handling, much more may be accomplished than by simple force, while risk is for the same reason avoided.

[The subject of sounding and finding the stone, so often missed by inefficient methods of searching, followed, after which the lecturer proceeded as follows]:—In operating by one sitting instead of several sittings, I am happy to tell you that no very great changes in the armamentarium are necessary. The work to be done is precisely the same as that we have always done before; the only difference is that larger stones may be attacked, and, as much larger quantities are to be removed, there is more work and harder work to be done than formerly. A stouter lithotrite, and one which will bite well into a hard stone and break it into fragments first, which by means of a less powerful but more easy working lithotrite may be crushed into débris, are mainly necessary. Hence, several patterns of the old fenestrated instrument which had been discarded as too heavy under the previous system may now be brought into use again, if fitted with the cylindrical handle, which by almost universal consent is adopted as the most convenient. For the fenestrated instrument does not become blocked by crushing, the small fragments being driven through the opening in the female blade. The flat-bladed

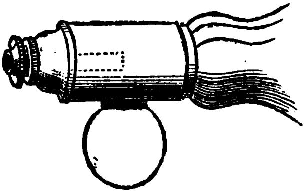


FIG. 3.—Clover's aspirator, with glass trap attached.

lithotrite, which hitherto has been much employed, should for this work be provided with a more sufficient opening in the female blade to prevent blocking, which renders it the most useful instrument in reducing fragments to débris adapted to pass a No. 15 or 16 evacuator. When a stone of medium size has been broken up, and by repeated crushings during some eight or ten minutes is obviously reduced, or nearly so, to débris, the lithotrite should be withdrawn, and an evacuating catheter Nos. 15, 16 to 18, according to previous examination of the urethra, should be introduced into the bladder, when urine and some débris will be expelled. It is, of course, sometimes necessary to divide the external meatus, but this is a matter of no moment. To this catheter the aspirator, of which there are several varieties, is next to be applied. Mr. Clover's original instrument is still a useful one, made rather stronger than formerly, and it may have a cylindrical receiver at the bottom of it if desired. (See Fig. 3.) Prof. Bigelow used as his first aspirator a strong large india-rubber bottle, from the top of which a long tube was attached to the catheter in the bladder, but he has modified this several times. A later form which he was good enough to show me, and which he exhibited last summer in London, is less simple in construction than any I should employ, although acting well with artificial fragments of uniform size in rigid vessels. The conditions under which we perform lithotrity in the human bladder, varying as it does every instant in size, and varying also in different individuals in regard to form, contractility, suppleness, &c., render experimental trials in rigid receptacles almost valueless, as I had occasion years ago to point out, being quite without analogy to the conditions of the organ in the living subject. Experience in the latter must guide us as to the best instruments of any kind to be used there. In designing a new aspirator at the outset for the one-sitting operation, I came to the conclusion that the following points are essential in the construction of an efficient instrument.

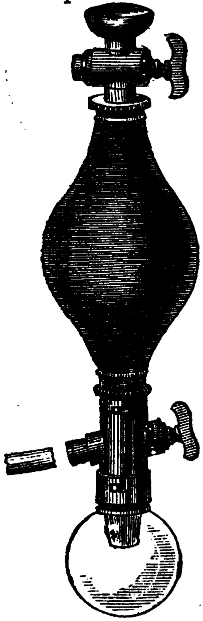


FIG. 4.—Original aspirator of Sir Henry Thompson.

1. It should be light and small, so as to be easily grasped and governed by one hand.

2. It should have an opening *at the top*, by which it can be filled with water, to which all air accidentally admitted, if any, will arise, and by which it can escape.

3. Its *lower part* should be connected with the evacuating catheter by the shortest route to the bladder, and in such a manner that the aspirator can be detached with ease, and without loss of any contained water in so doing.

4. There should be a trap into which all fragments must fall, and by which they are securely retained.

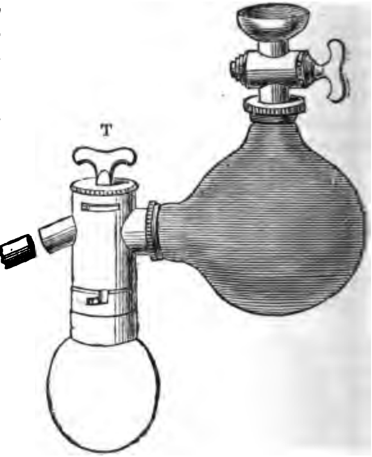


Fig. 5.—Recent change in the situation of the trap.

These conditions are all present in the aspirator which I designed at that time, and have used in almost every one of the 101 cases referred to. It is shown at Fig. 4, and was first described in this country in The Lancet of Jan. 17th, 1880. Quite recently a little alteration has been suggested, as shown at Fig. 5, which is identical with Fig. 4 in every particular, except that the cylindrical receiver, which is in Fig. 3 directly under the bottle, and may be slightly influenced by currents, is removed to the front of it, and is perhaps less disturbed by the current which passes over the mouth of the receiver. This change has been made by Messrs. Weiss and Son, and at their suggestion. In neither Fig. 4 nor Fig. 5 is any stand required, and the connection with the bladder is the shortest and simplest possible. The connection of the aspirator with the bladder is also cut off in both instruments by the taps T.

To continue the operation. The aspirator being attached to the evacuating catheter in the bladder, alternate pressure and expansion take place in obedience to the hand of the operator, who at the same time modifies the position of the catheter according to his judgment on observing the arrival of small fragments and débris in the trap. When much has come over, and the rattle and movement of fragments in the bladder have manifestly diminished, a large piece or two perhaps only being felt, which evidently cannot issue, the aspirator is removed, and a lithotrite introduced. If the fragments are not considerable, a flat-bladed instrument will probably dispose of the remainder; the aspirator is again applied, and the bladder is completely cleared. Of course if more stone remains, the process is repeated once or more. In from ten to forty or fifty minutes, however, a uric-acid calculus of very considerable size may be thus broken up and removed. Longer than fifty minutes I have never yet had occasion to prolong an operation.

After-treatment.—When the patient has fully recovered from the ether—if suffering severely, say three or four hours after the operation, a condition by the way which is quite uncommon, a hot hip-bath, as hot as he can bear it for fifteen minutes gives great relief. The treatment for the first three or four days in cases of large calculus is that of a mild acute cystitis; recumbent position, external warmth, frequent hot hip-baths, and small but frequent doses of solution of potash, just to neutralise the acidity of the urine. If the urethra is overstretched or bruised, an india-rubber catheter may be tied in for twenty-four hours or so, but this is not frequently necessary; while if the bladder had previously lost the power of emptying itself, such an inlying catheter for a day or two is mostly better than frequent catheterism.

There is one rather remarkable circumstance which I have met with in a considerable number of the cases referred to, and which I confess I am not quite able to account for. It happens in a large proportion during the three or four days immediately following the operation, that the relief is considerable, that the urine is clear, that the bladder tolerates a large quantity, and the aspect of the case is one of rapid and unchecked convalescence. But on the fourth or fifth day a little excitement appears, the bladder becomes irritable, the urine is cloudy, and after twenty-four hours or so subacute cystitis is established, often destined to be troublesome for a week or two. I have remarked these phenomena so frequently that it is impossible not to look for their occurrence; and I am satisfied now that they appear in spite of precautions, and are by no means necessarily the result of an imprudence on the part of the patient. The liability pointed out, however, indicates that it is most desirable to enjoin the recumbent position, confinement to the room, a warm temperature (in cold weather), and care in every particular of management for at least some days after crushing a calculus, whatever its size.

The next point to be considered is the mortality following the operation. As before stated, I have employed almost entirely the single sitting method during the last three years, operating on 101 consecutive cases of elderly men—that is, *on* 101 *separate individuals,* no one individual having required during that period to be operated on a second time. I have included no case of mere phosphatic concretions, which frequently require to be crushed for patients who pass all their urine by catheter, urine which is alkaline and liable to deposit phosphates. These are not cases of "stone in the bladder," and have no right to be so regarded. The mean age of the 101 individuals is over sixty-two years and a half. The number of uric-acid calculi is 61, of oxalates 4, of mixed calculi 8, and of phosphates 28. *The number of deaths is three only.* This is a better result than I have ever before accomplished. I have operated on a similar number by the old method, once, with five deaths, but the average of upwards of 400 cases reported to the Royal Medical and Chirurgical Society was 7½ per cent. mortality. I am sanguine enough, therefore, to hope, judging from this experience, that the mortality of the stone operations is reducible by the single sitting method materially below that of any previous one. Never, I believe, has it in the history of surgery hitherto fallen to the lot of any man to operate for the stone on 101 elderly male patients with only three fatal cases. The once famous 84 cases of Martineau, with two deaths, contained only 24 over 50 years of age (only 11 indeed over 60) *and in these the two deaths occurred.* A few among the rest were young men, *the majority were children,* and in these no deaths took place. Among Martineau's elderly patients the mortality was fully 8 per cent., an excellent result, and rarely attained by any of the older operators. The mortality now recorded in 101 consecutive adult cases of only 3 per cent. is, I confess, a better achievement than I had ever ventured to hope for.

I now come to the condition of the bladder subsequent to the operation, a question inseparable from lithotrity, long associated with cogent objections to it, and impeding its reception by surgeons during the earlier period of its history. For no one can deny that the operation of crushing the stone has heretofore been followed in certain exceptional cases by obstinate subacute cystitis with formation of phosphatic concretions, a condition demanding much patience and management to control, and which sometimes prove almost fruitless after all. The operations by repeated sittings, necessarily occupying with a large stone a period of fourteen to twenty-one days, and a much longer term in some hands, doubtless contributed to produce an unhealthy condition in some bladders which would not have been so affected had the stone been removed by lithotomy. Even after the cutting operation phosphatic cystitis is not unknown, although it is very uncommon. I have at this moment under my care an obstinate case which I cut eighteen months ago, the primary results being excellent.

Now, what is our experience of this complaint after lithotrity at a single sitting? It has been said, but I fear hastily, that this operation has banished these unhappy cases from practice. I regret to say, indeed, that so great an advance cannot be affirmed. I think one of the worst, one of the most persistent, examples of phosphatic cystitis I have seen of late years followed an operation in which I removed with great ease a small uric-acid calculus (eighty-four grains) in six minutes last October twelvemonth. Gradual improvement has taken place, but for a few months frequent micturition, much pain, and repeated production of phosphatic deposit were experienced. A similar case occurred about the same time, but this recovered soundly by the following spring. In four other cases of the series referred to there was persisting cystitis for three or four months after the operation, but not longer. All these are cases in which the calculus was uric-acid, and cystitis was not present before the operation. When the calculus has been phosphatic, removed from a bladder habitually emptied by the catheter on account of large hypertrophy of the prostate, and cystitis has pre-existed, it is almost invariably much lessened by the operation; but its continued existence in such a case to some extent is often a necessary concomitant of the organic changes in the organs, and involves no discredit to the surgical procedure.

But I think I am justified in concluding that the occurrence of the morbid condition of the bladder following lithotrity for uric-acid and oxalate of lime calculi in previously healthy organs, occurs less frequently now than heretofore. I venture to believe that the improvement in this respect may be considerable. In order to guard against the danger, our first care should be to avoid in every case where it is possible to do so the use of instruments, whether for crushing or evacuating, which over-distend and irritate the urethra and neck of the bladder. At least our rule must be not to employ larger instruments than the size of the stone demands. I cannot too earnestly warn the inexperienced lithotritist against the needless risk he incurs when, in presence of a small or moderate-sized stone (and the majority met with belong to one of these two categories), he uses the heavy lithotrite and the large evacuating tubes which have of late been introduced into this country. In order to remove two or three hundred grains of calculous matter from the bladder—and many calculi weigh less than a hundred grains, while all ought to be found before they attain that weight—it is wholly unnecessary, I will even say it is unwarrantable, to introduce lithotrites and evacuators with the diameter of No. 18 or 20 English scale into the bladder. I am certain that the mere splitting, for it is not dilatation, of the urethra and neck of the bladder, which sometimes follows the introduction of such instruments, has sufficed to produce symptoms often distressingly painful, sometimes obstinate in duration, occasionally fatal. Surely it is more prudent to bestow two or three minutes more on the work, to ensure more complete crushing, so that the débris may be removed by a tube of 15 or 16 English diameter, than to crush coarsely and use an evacuator which infallibly inflicts serious mischief in a certain proportion of cases.

I will briefly sum up our subject thus: Lithotrity completed at a single sitting is in experienced hands an operation unequalled in its safety for the patient.

It appears also to produce less subsequent persisting irritation of the bladder than the operation by several sittings.

No new form of instrument is required by this operation.

The value of the proceeding lies altogether in the removal of all the foreign matter from the bladder at once, so that nothing remains to excite inflammation in an organ already irritated by the process. And the less irritating the operation has been, the more certain and more speedy will be the recovery.

It should be employed by beginners only for calculi of moderate size when hard. If calculi are large as well as hard, a young surgeon will probably proceed more safely by lithotomy. In friable phosphatic calculi, size offers a much less serious difficulty. Lithotrity at a single sitting for a hard calculus, upwards of an ounce in weight, and *à fortiori*, when double that weight, certainly demands an experienced operator. *Lancet, Jan. 7 and 14, 1882, pp. 1, 47.*

60.—ON DIGITAL EXPLORATION OF THE BLADDER THROUGH INCISION OF THE URETHRA FROM THE PERINEUM.

By Sir Henry Thompson, Bart., F.R.C.S., &c.

The subject of digital exploration of the bladder has for some long time been regarded as a promising means of prosecuting diagnosis in exceptional cases, and still further as likely to constitute an efficient mode of treatment for some of them. Accustomed to use the index-finger as much as possible in a large experience of lithotomy in its several forms—lateral, median, bilateral, and medio-bilateral,—I have been in the habit of examining the neck of the bladder, and scrutinizing with much interest, its characters, often greatly differing as they do in the cases of elderly men. But it is only during the last few years that I have gradually realised the fact that it is possible, in not a few cases, to explore through a small perineal incision the whole, or nearly the whole, of the internal surface of the bladder with the index-finger. A necessary condition, of course, is that the bladder should be empty; and in that condition it is—as is sometimes perhaps not sufficiently recognised—no longer a cavity, as represented in diagrams, but a mere cul-de-sac at the end of the urethra. Taking it for granted that the incisions to be made, which involve only the urethra with the tissues between it and the surface, and not at all the bladder and prostate, enable the operator to place the last joint of his left index-finger within the neck and thus to draw it somewhat towards him, the exploration may be accomplished in the following manner. Maintaining his finger at the spot described, the operator should stand up at the foot of the table, a little to the left of the patient (who is in the lithotomy position), so as to make firm pressure with the right hand above the pubes, the resistance of the abdominal muscles being overcome by the influence of ether. He may now easily feel, unless the patient be very stout, the opposite side of the bladder coming into contact with the tip of his finger; and by concerted movements of supra-pubic pressure by the right hand, with slight movements of the left index in the bladder, almost every portion of the internal coat of the latter may be brought under examination. If the patient be thin, the proceeding is easy; it becomes less so in proportion to the depth of perineum and thickness of the supra-pubic coverings, both of which are increased by fat. In the latter case aid may be rendered by an assistant, who makes firm pressure with both his hands, but the operator should also employ, if he can, the concerted movements described.

Now, I think, it will be admitted that, however effective is the ordinary examination of the bladder by the sound and by rectal examination, and it amply suffices for the great majority of cases, there are still a few obscure examples of disease, respecting which, even in the most experienced hands, examination does not reveal facts which it is of the utmost importance to ascertain. This being so, the question arises, can digital exploration of the bladder be performed without much risk to the patient? I unhesitatingly answer in the affirmative. The results of external urethrotomy, on a grooved staff previously introduced, an operation largely performed by Professor Syme about thirty years ago—results which were jealously watched and closely scrutinised—go far to warrant this reply. But although the external incisions in his proceeding were almost identical in situation with those I shall advise for digital exploration, the internal incision in Syme's operation was made not only in tissues very often largely diseased, but was far more extensive than that required for the purpose now in view. To this evidence I may add that I have myself opened the urethra from the perineum many times for various purposes besides that of extracting calculus; and I do not recollect a single fatal result. Fifteen cases of external division of stricture on a staff are included among them. Next, the method of operating in order to reach the bladder from the perineum demands a few words. I think there is little ground for doubting that a vertical median incision—that is, in the line of the raphe—introduces the finger by the shortest and most direct route. The prominence formed by the operator's bended knuckles, while using his index finger as an explorer, lies equidistant between the nates, and is firmly pressed on the perineum in a straight line from the external surface towards the centre of the bladder. Any line right or left of the median must of necessity lead obliquely to the centre, and be a longer line, because it commences at a point on the external surface more distant from the neck of the bladder than is the raphe of the perineum. Accordingly I always adopt the central incision, using a median grooved staff, and a long, straight, narrow-bladed knife, with the back blunt to the point. Having placed the left index-finger in the rectum the knife may be introduced, edge upwards, about three-quarters of an inch above the anus, with or without a small preliminary incision of the skin (I prefer the former), until the point reaches the staff about the apex of the prostate gland, where it divides the urethra for half an inch or so, and is then drawn out, cutting upwards a little in the act, but so as to avoid any material division of the bulb. The left index-finger is now removed from the rectum, and following by the groove of the staff, slowly passes through the neck of the bladder as the staff is withdrawn, when exploration is made, as described above. A moderate division of the bulb when made in the middle line is not to be feared as a cause of hemorrhage, while an incision involving the side of the bulb, and nearer to the entry of its artery, is more prone to bleed freely. Hence, some have preferred another mode of incision in order to avoid it—namely, one of crescentic form, with the convexity upwards, just above the anus; the dssection to be carried transversely to the apex of the prostate, and opening the urethra there, as before mentioned.

Since the case of exploration which I performed in November, 1880, where I discovered and removed the tumour presented (together with the patient) at the Royal Medical and Chirurgical Society on the 11th of April last, I have made digital exploration of the bladder, in the manner above described, in four other cases. I append a brief note of each here, by way of showing its nature and the results, and I venture to think that they will be regarded as fully warranting a resort to the operation, and that the effect in three instances was a manifest gain to the patient.

Case 1.—A gentleman, aged forty-two; micturition frequent;

florid blood passed almost daily for nearly two years; occasional attacks of cystitis with phosphatic deposits. On June 27th, 1881, Mr. Clover gave ether. Having performed the operation described, I could detect neither calculous matter nor any structural change. A tube was tied in for five days, by which all the urine passed, and it was now free from bloody mucus. The tube was removed, and the wound soon healed. He was greatly improved, and is now living a very active life. He occasionally passes blood, and has some frequency of micturition, but is better than for years before.

Case 2.—A gentleman, aged fifty-four. I cut him for rather a large uric-acid stone in 1880; he made a fair recovery, but had cystitis afterwards, passing phosphatic concretions, and often blood; a condition resembling that which sometimes follows lithotrity. On June 16th, 1881, Mr. Clover gave ether. I explored for impacted calculus, &c., but could detect no deviation whatever from the normal condition. The tube was tied in four days, after which the wound rapidly healed, and no improvement could be noted. He is still under observation, and on the whole slowly improving.

Case 3.—A gentleman, aged sixty-eight. Has passed all urine for several months by catheter, and for the greater part of the time much blood and mucus have been mixed with it. He has also been diabetic for a considerable period. Before operating I had the advantage of Sir William Jenner's opinion about the case.—Feb. 10th, 1882: In exploring I found only a small flake of phosphatic matter, apparently adhering to the surface of the bladder, and nothing else abnormal. The tube was tied in eight days, after which the wound healed rapidly. He never had a trace of blood from this time, and the urine has been almost free from mucus; he requires the catheter precisely as before. On April 15th he returned to the country very ill and feeble with diabetes.

Case 4.—A gentleman, aged sixty. I formerly crushed a phosphatic calculus; he had long passed all urine by catheter with great pain and frequency, with much cystitis and recurring phosphatic deposits. His suffering was so severe that he begged the bladder might be opened.—March 20th, 1882: I made the exploration in presence of Dr. Chepmell, Dr. Barton Smith, and Mr. Hayward, and could not detect any abnormal condition. The tube was tied in for seven days, after which the wound rapidly healed. The patient uses the catheter now only about every three or four hours, the character of the urine is much improved; he is actively occupied, the relief to suffering being very considerable, and he is himself more than satisfied with the result of the operation.—*Lancet, May* 6, 1882, *p.* 724.

61.—ON THE OPERATION OF LATERAL LITHOTOMY.

By H. A. Reeves, Esq., Assistant-Surgeon and Teacher of Practical Surgery at the London Hospital, &c.

Perineal lithotomy is about the only operation in the whole range of surgery which is performed, as it were, in the dark. I propose to make lateral lithotomy, in the main, a day-light operation.

The steps of the operation as you saw me execute them were as follows:—The boy having been anæsthetised, I sounded, and having struck the stone, I passed a No. 6 rectangular catheter-staff, and again ascertained the presence of the calculus, then injected the bladder with half a pint of warm water. I prefer a rectangular staff and as large a one as the urethra can safely be made to hold, because the angle can readily be felt in the perineum before and during the operation, because it offers a straight guide into the bladder, and because a large staff distends the urethra and carries a groove more readily felt, and one in which it is easier to keep the knife during its passage onwards. In the second case a No. 6 passed easily, and I might have passed a No. 8 or 9 in the boy aged six. As you saw, there was not the least difficulty in passing the instrument, and if you pass it in the manner I did, from the left side of the patient, keeping the handle to the left until you get the end of the staff sufficiently far in the urethra to permit of the turn which will bring it safely into the bladder, there will usually be no difficulty in introducing it, and no fear, if you be gentle, of making a false passage.

While the staff was held in position the patient was tied up in the usual manner, and the pelvis was raised so that my eyes, when sitting, were on a level with the centre of the perineum. The penis and scrotum being well drawn up, I then made the usual first incision with this knife, which I had made specially, and which, please, observe. It cuts quite up to the point, and on its back, rather more than an eighth of an inch from the tip, is an ovoid button sufficiently large to be well held by the groove of the staff, and not large enough to impede the lateral inclination which I gave it before pushing it onwards to the bladder. One or two deeper incisions were made, and then I placed these retractors in position, one on either side of the wound. I then passed my finger till it rested just beyond the angle of the staff, and, guided by it, cut it till I exposed the groove and lodged the knife in it; then, being sure, by the senses of touch and sight, that I was in the right track, I steadily and firmly pushed the knife home to the top in the staff, and taking its handle in my left hand, I gradually depressed it, pushing it up and in with the knife, which was in the stop. The staff was then replaced, and as I withdrew the knife I kept it in its lateralised position and slightly enlarged the wound in the neck of the bladder and perineal tissues on its way out. Notice, that I not only cut *into* but also *out of* the bladder, and in my humble opinion this should always be done, if executed with care and the requisite amount of anatomical knowledge. If this step of the operation be properly performed there is no fear of having too small an opening, and thus leading to the risks of not entering the bladder, or pushing it away from the urethra, or making a pouch with the finger between it and the rectum. In the second case on which I operated about an hour after cutting the one of which I am speaking, I did not incise quite so freely, because the child was only half the size—viz., three,—and I did not feel justified in using the knife with the same freedom, and the result was that I had to enlarge the wound, which would only admit the tip of my index-finger, with a blunt-pointed bistoury. This only delayed the operation about a couple of seconds, and in both cases I caught the stone at once with the forceps, in the right axis, and readily extracted. The calculus in the first case was double the size of that in the second, and both were ovoid uric-acid calculi. In the second case a little *contretemps*, of no moment to the life of the patient, occurred, the nature of which I will explain to you presently. In the first case my index-finger had just room to pass comfortably into the bladder, and if care be taken that this be always the case I believe all the difficulties connected with entering the bladder and extracting the calculus will be overcome. Always feel the stone with your finger before withdrawing the staff, and be not alone satisfied with feeling, but endeavour to do that which I did in these cases, and have it beneath the pulp of your finger, and keep it there before passing the forceps or scoop on to it. In both cases I tried at first to extract with my right index-finger in the rectum. However, I did not waste a moment directly I felt it was not quite easy, but introduced a small pair of calculus forceps, and you saw how easy the extraction was. In the first case I passed my finger into the bladder between the groove of the staff and the neck of the bladder, and in the second I found it easier to get my finger in above the staff. I mention this because some writers lay stress on the way in which you are to pass your finger into the bladder. In my view it is of little moment, because if you have made the opening at the vesical neck sufficiently large your finger will have no difficulty in following the groove of the staff into the bladder. There will be no fear of making a pouch with your finger in the loose tissue beneath it. You will have noticed that there was no bleeding in either case, and there is every reason to believe they will both do well. [Both cases recovered very satisfactorily.]

A few words about one or two of the preliminary steps: I allude to injection of the bladder, which some writers do not think necessary, and to pulling up of the penis and scrotum. I think the bladder should be fairly distended, because the weight of the water helps to steady it, because the recto-vesical pouch of peritoneum is pushed up (although this should not be in danger, yet it has been wounded more than once), because, as these cases tend to show, the walls of the bladder do not always collapse over the stone after the urine gushes out, and also because the base of the organ is brought lower down when it is moderately distended. The pulling up of the penis and scrotum is valuable because this proceeding stretches the urethra over the groove of the staff; it pulls the vesical neck lower down, and the urethral bulb is drawn up out of the way of the incision. The pulling up of the scrotum has the advantage of rendering the skin tense, of facilitating the first incision, and of permitting the angle of the staff to be better felt before operating.

The difficulties, then, of perineal lithotomy are, as you have now made out, threefold: first, to find the groove of the staff; second, to keep the knife safely in the groove, and push it steadily on to the stop at the vesical end of the staff; and, third, to extract the calculus. I have demonstrated to you practically, as well as described theoretically, how these are to be overcome; but I wish to explain to you some further steps which I was prepared to adopt in order to render every step of the operation as safe and simple as possible. These retractors, which are of a somewhat special shape, I had made for these operations, and if I had wished I could, with my thumb and finger, have pushed them gradually onwards towards the vesical orifice on either side of the staff, so as not only to obtain a view of the groove in the greater part of its extent, but, if necessary, to pass one of them along the groove into the bladder after the knife was withdrawn, and then by turning the handle to the floor I could have stretched the vesical opening enough to allow of the introduction of the index-finger. In fact, had the opening not been of sufficient size, and had I not liked to have made further use of the knife, I should have had three courses open to me. I could have adopted the plan just described, or passed a grooved director, and then my finger between it and the staff, or have guided a pair of dressing forceps along the groove, and if I could not insinuate my finger between it and the staff, and had chosen to dilate, I could have done so with the forceps, and perhaps have got my finger in between its expanded blades. But these plans are best avoided, and they can be, by not being afraid of cutting with due judgment, skill, and anatomical knowledge; and if you have once made sure that you are in the groove, a bold incision is one—perhaps the greatest—secret of successful and brilliant lithotomy, always assuming that the cut does not exceed the limits which experience has approved.—*Lancet, Dec.* 3, 1881, *p.* 939.

62.—ON EARLY TREATMENT OF PROSTATIC OBSTRUCTION.

By REGINALD HARRISON, F.R.C.S., Surgeon to the Liverpool Royal Infirmary.

It may be generally stated that of males who have passed fifty-five years of age, about one third sooner or later have enlargement of the prostate; of these about one-half suffer therefrom, though so long as micturition is efficiently and painlessly performed, there are seldom grounds for complaint.

It is exceedingly interesting, as indicating how relief may be afforded, to analyse the cases where the prostate is large but does not obstruct; there are at least two conditions explanatory of non-interference with micturition under these circumstances. First, where the hypertrophy is towards the rectum and the relations of the prostatic urethra are not altered; and second, where the hypertrophied gland is lobulated and channels are left between the masses along which urine flows without interruption. A careful consideration of these conditions has suggested that they are capable of artificial production to a useful extent.

The teaching of the present day is, however, to the effect that mechanical treatment is not to be employed until either retention occurs or the bladder becomes inflamed, then such means may be resorted to. It was asked why the same treatment should not be applied as in the case of urethral stricture. The objection generally advanced is that irritation will be produced. It was urged that there was no evidence in support of this objection. On the contrary, the prostate was about the most long-suffering organ in the body, and though it was subjected to a great variety of mechanical expedients, in lithotomy and other operations, it rarely became inflamed. It might just as well be said that because strictures were found occasionally to be exceedingly irritable, treatment was to be postponed until retention of urine or cystitis were provoked. But if intolerance to early mechanical treatment were proved, it was only postponing the day until the necessity was greater and the difficulty more apparent. If there was danger of irritating the prostate, it was none the less because its size was larger.

With the view of obtaining similar conditions to those occurring naturally in large prostates, where there is no interference with micturition, a mode of treatment with specially adapted bougies was described. The instruments are gum-elastic, two to four inches longer in the stem than usual, with an expanded portion an inch from the tip, which is made to enter the bladder. In this way the prostatic urethra is subjected to pressure on the insertion and withdrawal of the instrument.

As a rule, if dilatation is not too rapidly proceeded with, no irritation is aroused. On the contrary, greater toleration of urine follows, owing to the ease and completeness with which the bladder is then emptied.

In a few persons it became necessary to establish a state of instrumental toleration, the frequency for doing this, as a rule, depending more on the manipulator than on the instrument. In some individuals intolerance of urethral interference was entirely due to the condition of the urine. Such sensitiveness had been traced to the presence of uric-acid in unnatural quantities and form. On the correction of this, patients previously intolerant of instruments were found capable of undergoing the necessary mechanical treatment with the greatest advantage.

In advocating the early treatment of prostatic obstruction by the means referred to, the author had already had sufficient proof of its efficacy. He had demonstrated that the regular use of the dilators was capable of so moulding the enlargement as to prevent obstruction. Cases were under observation where the symptoms indicated that an impediment to micturition was commencing to form. Such patients in this way regained the power they were beginning to lose. In bringing forward his views on the subject, Mr. Harrison did so with the feeling that little had been yet done towards preventing the progressive development of a condition which was often followed by very distressing, and sometimes embarrassing, results—results which we knew of and stood by to palliate, though we had hitherto been helpless in preventing them.—*Medical Press and Circular, March* 15, 1882, *p.* 225.

63.—ON TAPPING THE BLADDER FROM THE PERINEUM THROUGH THE HYPERTROPHIED PROSTATE.

By Reginald Harrison, F.R.C.S., &c., Liverpool.

In the Brit. Med. Journal of Dec. 24, 1881, I reported a case where I had performed puncture of the bladder for retention of urine from the perineum through an enlarged prostate. I am now enabled to complete the history of that case.

After the bladder was tapped, for about six weeks my patient

Fig 1

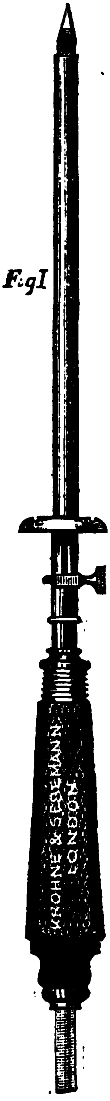


Fig 2

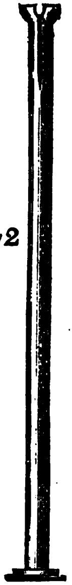


Fig 3

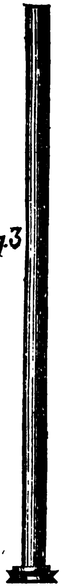
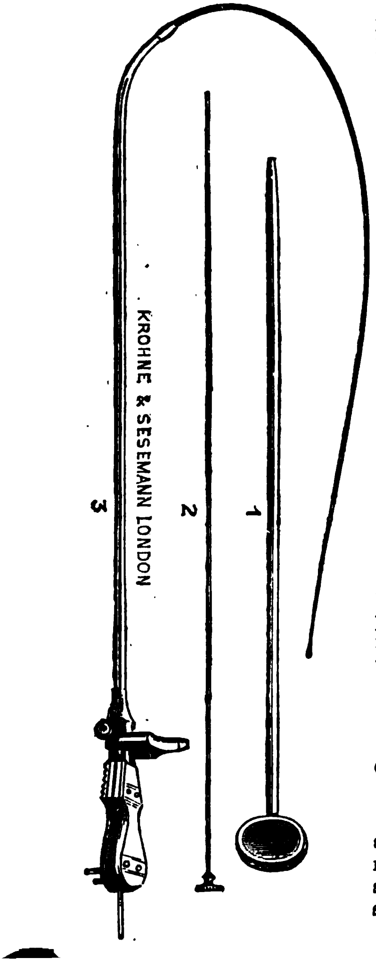


Fig. 1 represents the Trocar with Canula complete. A piece of India-rubber tubing is to be attached below the collar of the canula on withdrawal of the trocar, to convey the urine into a receptacle.—Fig. 2. Silver canula, which is introduced in tube of Fig. 1 to form a probe end, and for retaining it.—Fig. 3. A plain silver tube with which to clear tube (Fig. 2), in case of it being blocked up.

passed urine through the prostatic canula. His health rapidly improved, and he was able to go about as if nothing ailed him. Then it was noticed by us that urine in gradually increasing quantities began to flow through the natural passage, so much so as to lead me to infer that, for some reason or other, the prostate was ceasing to obstruct micturition.

On Jan. 28th, I removed the prostatic canula; the puncture healed in the course of a few days, and with this the bladder gradually proceeded to recover its natural function and power. It must be remembered that, for over two months, I had resolved the patient's act of micturition into the simple mechanism of turning a tap. During the day-time he now holds his urine for two or three hours, and at night he gets up twice, and sometimes thrice, to micturate. It may be said that all functional symptoms of enlarged prostate have ceased to exist.

On the day of his admission into the infirmary, when the puncture was performed, rectal examination, made both by my house-surgeon, Mr. Laimbeer, and by myself, left no doubt that the prostate was greatly enlarged. Upon this point there could be no other conclusion. Now we find that the enlargement has almost disappeared; in fact, the finger, by the rectum, discovers but little that may be regarded as abnormal—a strong contrast to what previously existed. Here, then, we have a case where a surgical proceeding on an enlarged prostate was followed by its rapid atrophy, a result which, so far as I know, has not been noted before. It may be urged that rectal examination affords evidence that only one side of the enlarged gland has thus become reduced; it must, however, be remembered that a return of the power and function of micturition warrant a conclusion that a corresponding change has taken place in the vesical aspect of the gland.



Though I am only at present able to narrate one case, it will be taken for what it is worth, and as capable of being imitated.

I desire to record it as bearing upon the radical treatment of an affection for which little has hitherto been done.

Appended is a short description, with an illustration, of the instruments I have devised for this operation, which have been made for me by Messrs. Krohne and Sesemann.

The trocar is hollow, and urine escapes through it as soon as it enters the bladder. The shield on the tube (Fig. 1) is movable, and can be fixed at any desired position to suit the varying thickness of prostate and tissues. The instrument is adapted for other purposes than that referred to in this communication.—*British Medical Journal, April* 8, 1882, *p*. 498.

64.—DILATOR OF THE URETHRA IN STRICTURE.

The accompanying figure represents Mr. Reginald Harrison's modification of Holt's dilator for stretching the urethra in cases of stricture.

Mr. Harrison has added a pilot

bougie, which he finds of great value in cases of tight or tortuous stricture. He has increased the number of dilating rods to eight, in order that the process of stretching may be gradual; and, to avoid jerking when the several rods are introduced, the separation of the two portions of the instrument is controlled by a spiral spring instead of a screw. The dilator, before a rod is introduced, is No. 3 English gauge; the largest dilating rod brings it up to rather over No. 12.

[A description of the manner of using the instrument will be found in the "Synopsis" of this vol. of the *Retrospect*.]

The object which Mr. Harrison has in view in the use of this instrument is to gradually stretch the strictured portion of the urethra, not to rupture it. He compares the process to that to which new gloves are submitted; the stretcher is introduced into each finger of the glove, and, if the process be properly carried out, there is no tearing or splitting.

In the figure, 1 represents the dilating rod; 2, a fine stylet for the catheter; 3, the dilator with a movable pilot bougie and spiral controlling spring.

The instrument was shown by Messrs. Krohne and Sesemann at the International Medical and Sanitary Exhibition.—*British Medical Journal, Dec.* 31, 1881, *p.* 1060.

65.—NEW METHOD OF URETHRAL IRRIGATION.

By WALTER WHITEHEAD, F.R.C.S.E., F.R.S.Ed., Surgeon to the Manchester Royal Infirmary.

Irrigation, judiciously employed in suitable cases and at a proper stage of the complaint, is unquestionably a valuable adjunct to the treatment of gonorrhœa. In the first place, the prolonged stream of water effectually removes the inflammatory products of the disease, which obstinately and intimately adhere to the surface of the urethra, and protect the mucous membrane from the immediate influence of the remedies we endeavour to bring into action, either through the urine medicated by the administration of balsamic drugs, or by the more direct influence of antiblennorrhagic injections. The tenacity with which the secretions adhere has, no doubt, much to do with the difficulties encountered in the treatment of gonorrhœa. It must be remembered that the urethra in health is an elastic tube tolerant of reasonable dilatation; but when invaded by inflammatory changes, it is converted into a comparatively unyielding tube, intolerant of any force having a tendency to expand its calibre; consequently, irrigation, to be safe and successful, must be conducted on a principle consistent with the laws of hydrostatic pressure; and as we propose to use irrigation in a disease which involves conditions opposed to distension, it is required that the mode of irrigation, whilst insuring a continuous and copious stream of fluid, should be free from any undue tendency to stretch the walls of the urethra.

These conditions I have obtained by the following apparatus and mode of precedure.

I have had catheters made, some in vulcanite and others in soft India-rubber, by Messrs. J. and W. Wood, of King Street, Manchester, with a deep spiral groove from tip to stem on their outer aspect.

The catheter ends in a hollow bulb perforated with two large apertures directed backwards. The other extremity of the catheter is adapted for the ready attachment of India-rubber tubing. The groove in the catheter is made with a depth double the capacity of the central tube, so that the facilities for the return of injected fluids are greater than the requirements; and, in consequence, there is no excessive pressure on the urethra, the outflow being more than ample for the inlet of the irrigating fluid.

In using the irrigator, it is passed to different lengths of the urethra, after the manner of an ordinary catheter, according to the nature of the case, the aim being to get well behind the furthest limits of the diseased surface. The distance can be roughly estimated by the clinical features of the case, and by testing how far back the discharge can be pressed out of the urethra.

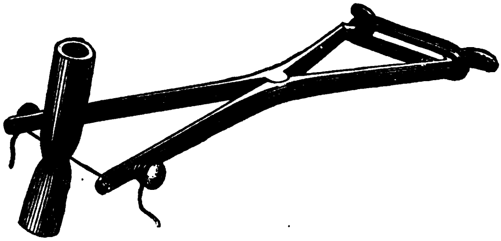
When introduced, a length of India-rubber tubing is attached to the irrigator; and the other end of the tube, passed through a leaden weight, is placed in a vessel containing the fluid to be used, and elevated above the body of the patient. The tube being made to act as a syphon, the strength of the stream can be regulated by the height of the vessel, and by this means a simple and complete control can be exercised over the force brought to bear upon the surface of the urethra and the feelings of the patient. The bulbous extremity of the catheter practically restricts the action of the fluid to the urethra in front, and prevents any of the injection from passing backwards into the bladder. The direction of the stream is also influenced by being diverted forwards within the hollow bulb. The irrigation of the urethra being regarded merely as a measure preliminary to treatment, it remains, after the mucous membrane has been thoroughly cleansed, to apply one or other of the various topical solutions we are in the habit of using; and this can be easily accomplished by connecting a syringe to the irrigator after the rubber tubing has been removed.—*British Medical Journal, April* 8, 1882, *p.* 497.

66.—ON VARICOCELE AND ITS TREATMENT.

By H. Royes Bell, F.R.C.S.Eng., Surgeon to King's College Hospital.

Varicocele is most common between the ages of fifteen and twenty-five; Curling gives twenty-six cases out of fifty; Landouzy twenty-nine out of forty-five at that age. Landouzy, Bryant, and Gould give cases in infants and boys under ten years of age. Sir Astley Cooper, who thought so lightly of the disease, was induced in some cases to perform a severe and uncertain operation for its cure—namely, excision of a portion of the scrotum. Mr. Key actually removed a testicle for a painful varicocele. Sir W. Fergusson cured a varicocele by thrusting a red-hot awl into the veins, when requested by the patient to remove his testicle. There is no relation between the size of the varicocele and its painfulness. M. Gosselin found no spermatozoa in a case of obliteration of the right vas deferens and varicocele on the left side. Mr. Curling also reports two similar cases; in one there was a double varicocele with soft and small testicles; in the other the right spermatic duct was blocked after epididymitis. On the left side there was a varicocele; both sides were softened. The knotted, thickened veins are obvious to the sight and feel, become small when the patient lies down, and dilate when he coughs or exerts himself; the testicle reaches at times half-way down the thigh. When the swelling has disappeared on lying down, the surgeon is to make gentle but firm pressure on the external ring. If the case be one of hernia, the swelling will not reappear; but if a varicocele, it will fill from below in spite of the pressure. A congenital hydrocele returns slowly, is transparent, and fluctuates.

The palliative treatment consists in tonics, aperients, and the cold douche. The parts are best supported by Mr. Keetley's suspender. Mr. Wormald used a wash-leather-covered silver ring, soft enough to allow its sides to be squeezed together, to support the scrotum. Mr. Curling speaks highly of a Moc-Main lever truss, which makes pressure on the veins at the external ring, and is to be applied before the patient rises. It relieves the neuralgic pain, and the testicles in some cases have increased in size. M. Breschet used to compress the spermatic veins and the scrotum by means of two pairs of screw forceps with protected teeth, which produced a hard dry slough in twenty-four hours, and coagulation of the blood. Landouzy hollowed out the forceps so as to protect the margin of the skin. Sir A. Cooper and Sir B. Brodie both discarded the old operation of cutting down on the veins and tying them, as it was attended by phlebitis and death. The saying that history repeats itself is undoubtedly true of surgery, for some surgeons isolate the veins selected for obliteration by dissection, tie them in two places with carbolised catgut, divide them between, operating with antiseptic precautions. Mr. Henry Lee now removes a portion of the scrotum over the varicocele, compresses the veins temporarily to prevent hemorrhage, then divides and seals their cut orifices by applying a black-hot cautery for six seconds; the compression pins and threads are then removed, and the skin sewn from below upwards. Some surgeons are content to ligature the veins subcutaneously with catgut. M. Ricord's operation is performed by passing subcutaneously two double threads, one in front, and one behind the veins between them and the vas deferens. The ligatures pass through the same openings in the skin, and present a loop and two ends at each opening, the two ends are put through the corresponding loops, and traction made by a horseshoe-shaped *serre-nœud*. Mr. Tufnell, of Dublin, uses wire, and fastens a piece to the loop to act as a tractor to facilitate its removal. M. Davat was content to pass a single needle subcutaneously between the veins and the vas deferens, and twist over a figure-of-eight of soft silk; the ends of the pins were removed by wire nippers. Mr. H. Lee improved on this operation by passing several needles instead of only one, and dividing the veins subcutaneouly on the third day, removing the pins on the fourth; subsequently, to avoid what was practically a second operation, he divided the veins at the first operation. Sir William Fergusson used formerly, and Mr. Henry Smith still performs, the old operation by passing two or more harelip pins between the veins and the vas deferens, then compressing the vein by twisting a figure-of-eight of soft silk over the pin ends, having first protected the skin by passing a narrow strip of lint around the pins. The pins are removed in a few days, when they have set up sufficient action to consolidate the parts and obliterate the veins. Mr. Curling introduces two pins between the veins and the vas deferens, and protects the skin by means of two pieces of cardboard, over which he twists the soft silk thread. A tenotomy knife is carried beneath the veins, which are then divided subcutaneously; the pins are removed on the sixth day. Hydrocele occurs when the pins are placed near the tunica vaginalis; suppuration has occurred, and Mr. Lee has reported a case of hemorrhage. M. Vidal de Cassis passes a pin perforated at each extremity between the veins and the vas deferens, and then introduces a silver wire subcutaneously by means of a needle in front of the veins passing through the wounds made by the pins. The ends of the wire are to be passed through the corresponding holes in the pin, by twisting which on itself the wire is wound round the pin and compresses and rolls up the veins. The wire is to be tightened from day to day. Mr. Erichsen passes a silver wire subcutaneously round the veins, which are constricted and finally obliterated by tightly twisting the loop. An operation which requires to be repeated as it were daily is faulty as a surgical proceeding, and one dreaded by the patient. Mr. J. Wood passes an iron wire around the veins in the usual way, the ends are then passed through a large eye at right angles to the end of an iron rod about the size of a stout knitting needle, which has at the other end an elongated horseshoe-shaped spring of steel to which the wire is attached. The spring acts continuously on the wire, which would, if desired, cut through the veins. To avoid the ulceration caused by the direct pressure of this instrument on the skin, I have devised a small traction instrument which is used in the following way. Pass an unthreaded needle with an eye at its point between the veins and the vas deferens; knot a loop in a piece of stout silk; pass one end of it through the eye of the nævus needle, which is then to be withdrawn and passed, without unthreading it, subcutaneously in front of the veins; now unthread the needle and pass the end of the disengaged thread around the free thread. Do not withdraw the needle, but pass the free end of the thread through its eye; it is now to be withdrawn, the veins will be tied subcutaneously, the ends of the thread are to be passed through the eyes of a small instrument which has four arms that unite at what is called a circular joint. Traction is made and regulated by twisting a number of turns of an india-rubber band round the distal end of the tractor. The tractor can be readjusted at any time if it gets loose, without pain; it pulls away from the veins and skin, and the small holes made by the passage of the threads soon heal. There is no rule as to the time ligatures are to be left round the veins, but the usual time is eight or ten days. I have met with good results after the operation with the elastic ligature, which may be passed round the veins sub-



cutaneously by means of a large sewing needle; the elastic cord is to be stretched to the utmost, and then clamped by means of a leaden circle, which is pushed well home. The galvanic cautery is used by Mr. Pearce Gould, of the Westminster Hospital, for the cure of varicocele. The veins are isolated from the cord, a narrow-bladed knife passed through on the flat between the veins and the vas deferens, a needle threaded with a platinum wire is then passed along the tract of the knife behind the veins, and subcutaneously in front of them. The wires are then connected with a galvanic écraseur, become red hot, and in a few minutes cut through the veins. The operation is bloodless and radical in its nature, the skin is prevented from being charred by dropping a little water at the point of exit of the wires. A small nodule is to be felt later on where the veins are divided. Varicocele is attended at times with a good deal of neuralgic pain, especially after much standing or muscular exertion, which distracts the patient's attention, and causes more mental worry than its presence warrants. In adults the testicle may feel softer than normal, but in young cases Mr. Gould states that the testicle is small and undeveloped, not wasted. The operation for the cure of varicocele is not formidable, is attended by little pain, and is free from danger.—*Lancet, January* 28, 1882, *p* 133.

67.—ON THE RADICAL CURE OF VARICOCELE.

By Reginald Harrison, F.R.C.S., &c., Liverpool.

I should like to describe a mode of operating which, without prejudice to other procedures, has given me results which have been very satisfactory. It consists in exposing the cord by a vertical incision about an inch in length and separating the veins which are most prominently varicose. These are each tied in two places with a catgut ligature. Usually three or four main veins require to be so dealt with. In addition, it will be observed that there are a number of small veins in plexuses or bundles, especially in close proximity with the epididymis, which cannot be obliterated in this way; these I destroy with a few light touches of the thermo-cautery, taking care, while this is being done, to protect the vas deferens by holding it from behind, between the thumb and index finger of the left hand. The operation is conducted antiseptically. No sutures are introduced, and the wound is left to heal by granulation. I have now been performing this operation for some years, and I am not aware of an instance in which there has been a return of the varicose condition of the veins; the cicatrix which results is of a contractile nature, such as usually follows wounds inflicted by burning, and gives a feeling of firmness to parts which previously were complained of as being unnaturally lax. It is, I am sure, a matter of great importance, in view of preventing a return of the disease, to destroy the small varicose veins, which can only be seen and not felt. If this is not done, obliteration only means driving the disease into new channels, already preparing to dilate.

If antiseptic surgery has done nothing more than give precision to surgical operations, and careful attention to detail in their after-treatment, it has effected much. If it is wished to deal with a part that is diseased and to obtain the fullest success, it almost amounts to a condition that it should be seen, provided that this introduces no additional element of danger.

I have operated on the spermatic veins in this way a great many times. In one instance only was any delay or danger imported into the case. There was an attack of erysipelas, which was directly traceable to the patient sitting on a draughty watercloset very shortly after the operation was performed. Though here there was delay and some danger, the result was satisfactory. As to the necessity for performing an operation with a view to effecting a radical cure, the sensations of patients, in addition to changes taking place in the testicle, are sufficient to indicate this. Hence, in deciding what is to be done, it is of importance to have under notice the various procedures having for their object the same result.

In the reports of the Army Medical Department I find that the number of recruits rejected on account of varicocele was as follows:—

Date.	Men examined.	Rejected for varicocele.	Ratio per 1000.
1878	43,867	574	13·08
1877	43,803	676	15·43
1876	41,809	587	14·04
1875	25,878	326	12·60
1874	30,557	371	12·14
1873	24,895	324	13·02
1872	28,390	536	18·88
1871	36,212	670	18·50
1870	38,408	801	20·86
1869	17,749	447	25·19

As these figures are taken from males approaching the prime of life, they not only give some indication of the prevalence of the disease, but further warrant the careful consideration of all methods having for their object its radical cure.

I see that recently Dr. Henry, of New York, and Dr. Levis of Philadelphia, have revived the old practice of removing with a clamp portions of the scrotum for the purpose of giving a permanent support to the enlarged veins of the cord, with the object, I presume, of effecting their obliteration. Though I have had no experience in this mode of operating for varicocele, I am rather disposed to doubt its efficacy by reason of the remarkable reproduction, if I may use this term, of the scrotum after it has been completely destroyed by sloughing, the result of urinary extravasation.

I recorded a case in 1875 of sloughing of the scrotum where the testicles were exposed and eventually covered in by granulation. This patient, Charles W——, happens now to be in the infirmary under my care for stricture. His scrotum, which was almost completely destroyed by sloughing, has been, as I pointed out to my class, entirely reproduced ; if anything, it is rather more lax than natural. This is by no means a solitary example.—*Lancet*, *March* 25, 1882, *p*. 477.

DISEASES OF THE SKIN, ETC.

68.—ON THE TREATMENT OF ECZEMA.

By P. H. PYE-SMITH, M.D., Assist.-Physician to Guy's Hospital.

With respect to the treatment of the various forms of eczema, the first condition of success is, I believe, to recognise that the condition is one of ordinary inflammation of the Malpighian and papillary layers of the skin, not "constitutional" or "diathetic," any more than inflammation of the kidneys or of the stomach.

Next, we must look carefully for sources of irritation. It is remarkable that ordinary squalor and neglect produce pustular eruptions, but seldom true eczema; and vermin lead to prurigo or to urticaria, but seldom to eczema. Nor is eczema produced by animal poisons, as are the pustular and erythematous eruptions which we see in the hide workers of Bermondsey, and in butchers. Eczema is the result of the irritation of sweat or of friction, or of exposure to fire, to hot sun, or cold wind. Still more frequently it is produced by the mechanical or chemical irritants used in various trades; as the water in which the washerwoman's hands are kept half wet and half dried, and the coarser kinds of sugar handled by grocers. Eczema of the anus and genitals again may sometimes be traced to want of scrupulous cleanliness in adults as well as in children.

We must also, in all cases of pustular dermatitis, remember the contagious property of pus, varying greatly in degree, but never to be lost sight of. Many cases of impetigo which seem at first sight to be idiopathic, can be traced, especially in children, to inoculation by the nails from a few pustules produced by scratching an occiput infected with pediculi, or to similar inoculation from a whitlow, from an inflamed phimosis, or from the sores produced by accidental injuries. The spread of scabies itself is not only due to the direct irritation of the acarus and the secondary irritation of the patient's nails, but also to the contagion of the pus. So, again, we see furunculi and ecthyma appear in crops from a single primary source of suppuration.

In most cases, however, and particularly those of the most typical kinds of eczema, we find no traumatic or infective origin. They are strictly idiopathic. Between the purely accidental dermatitis, to which all skins are liable under irritation, and the purely idiopathic eczema where no irritating condition can be found, there is every gradation. As in catarrhal pneumonia, as in dyspepsia, it commonly takes two to make a quarrel. Some persons are exposed to cold yet do not cough, others eat too much and too fast yet suffer no remorse. All I venture to maintain is that the difference between one person and another is not in the "constitution" or "diathesis," but in the anatomical structure (hereditary or otherwise acquired) of the lungs, or of the stomach, or of the skin.

We must then, regarding eczema as dermatitis, treat it like other inflammations, and first and most important is *local* treatment.

We relieve the inflamed skin from the friction of the clothes, and as much as possible from that of movement. We protect it from air by rags soaked in lotion, or by smearing it with unguent, or by dusting it with an indifferent powder. And thirdly, we must protect it from water, or rather from the change from moist to dry by evaporation, which is the result of washing.

The late Prof. Hebra published not long before his death a characteristic and amusing lecture on the deleterious effects of water upon the skin. Few of us can be convinced that the daily tub will do healthy English skins anything but good. But there is no doubt that not only soap and water but water alone may be an irritant to an inflamed skin, just as food which is suitable to the stomach in health may be an irritant in gastritis. If we keep an eczematous surface under water, it is soothing so long as the temperature is the same; a continuous bath is sometimes excellent treatment, and there is no objection to the water dressing except from the heat it maintains. But we shall do wisely to forbid washing in the ordinary way in most cases of eczema. Oatmeal, or gruel, or size baths are soothing as well as cleansing if of proper temperature (about 90° F.), and if continued for at least a quarter of an hour; but they are more useful in cases of prurigo, especially infantile prurigo, than in eczema, and should only be used in this disease when the surface affected is large and the secretion free. In eczema, and especially in impetigo of the scalp, the hair must of course be cut short, or in severe cases shaved, the crusts softened with poultices and prevented from re-forming by oil, and the scalp kept clean with equal parts of strained white of egg and water; even this should be sparingly applied and scrupulously dried.

What we want to procure is uniformity of condition. All irritants to living tissues, mechanical, chemical, or what not, are more or less sudden changes. It is possible to heat a frog's muscle until the myosin is coagulated without producing a twitch, or to introduce a constant voltaic current into a nerve, gradually to increase its strength until it much exceeds that of an efficient stimulus, and gradually to diminish it until it can be withdrawn altogether, yet without a negative variation being produced. It is the rapidity of a change, not its amount, which acts as an irritant, whether in the normal or the morbid department of physiology.

Poultices or water dressing with gutta percha or india-rubber or goldbeater's skin are almost always hurtful from the heat which is produced, whether first applied cold or hot. Nor do I think that we gain by using alkaline water, as used to be the custom at St. Louis. Theoretically, one would recommend "the normal salt solution" of the laboratory with enough carbonate of soda to make it faintly alkaline. But, practically, it is difficult to prevent even half per cent. alkaline solutions from causing irritation to a raw eczematous surface. Except as prolonged baths or in exceptional circumstances, it is better, I believe, to use moist applications in eczema only as medicated lotions.

Before leaving the subject of water in eczema I may remind the less experienced reader that the cases of general, irritable, weeping eczema are, in old persons, and occasionally in infants, fatal. In these cases continuous luke-warm baths seem to be indicated, but whether really "exhausting" or not, they are supposed to be so, and must therefore be administered with caution. One of my first cases of eczema was in a stout, handsome, healthy old gentleman, with pink skin and silvery hair, whom I saw with Mr. L—. It was widely distributed and excessively irritable. Fresh from Vienna, I ordered a continuous bath, as I had seen it used by Hebra. Great relief followed; but an older and more sagacious physician, who was afterwards called in, while not attempting to cure the eczema, predicted a speedy and fatal result, which soon after happened, and I have not met Mr. L— since.

A short time afterwards, when medical registrar in this hospital, I found, just admitted under Dr. Wilks's care, a patient with extensive and irritable and weeping eczema, also a man above seventy, with clear pink complexion and abundant white hair, and also with a history of gout, though, as in the other case, without evidence of renal disease. I made an unfavourable prognosis of the case. But the patient recovered. I have at the present time an old gentleman of eighty under treatment with extremely obstinate pruriginous eczema, and he appears at present to be equally unlikely to part with his eczema or with his life.

The drugs which we find most useful in controlling local inflammation are those which belong to the group of astringents—zinc, borax, alum, chalk, tannic acid, silver, lead. Of these lead is the most generally and deservedly employed in the treatment of eczema. Zinc and borax appear to have the special additional merit of diminishing irritation. Nitrate of silver is only suited to circumscribed and chronic patches of inflammation.

We have three modes of applying these drugs, as dry powders, in solution, and in suspension or chemical combination as unguents, oleates, or plasters. The general rule I learned from the late Dr. Hughes Bennett is an excellent one: lotions to wet and ointments to dry eruptions. If greasy applications are made to a profusely secreting *Eczema madidans* the discharge washes away the ointment, so that the lead or chalk or zinc never reaches the diseased surface any more than if it were applied over the thick crusts of *Impetigo larvalis*. On the other hand, if lotions are placed in contact with intact epidermis, the horny scales, rendered more water tight by the sebum which covers the surface of the healthy skin, form an almost impenetrable barrier to the action of the drug in solution.

There are, however, exceptions to the rule. Lead lotion is often found to be the best application in the early stages of eczema while still erythematous and in some of its most pruriginous dry forms. Lotions are indicated in hot weather when the skin sweats freely, and ointments in winter when there is no fear of their turning rancid. Lotions are easily applied to infants by the nurse, and to the face and upper extremities by the patient himself; but to be efficient they must be constantly renewed and the surface as lightly covered as possible. Hence they are less applicable to parts of the body which cannot readily be exposed and handled. For the same reason one more often prescribes ointments as a vehicle with hospital out-patients, and those who are about all day, and lotions with in-patients, and those who can or must lie up at home and devote themselves to their cure. Lastly, we meet with certain cases in which our patients assure us that either lotions or ointments always disagree with them, and I have too often verified this assertion to neglect it.

When there is much clear serous effusion, and especially in eczema of the folds of the limbs, powders are often better than either lotions or unguents. Finely powdered chalk, oxide of zinc, or zinc and starch, dry up such weeping surfaces and form a false scab under which the healing process goes rapidly on. The intertrigo of infants and of the breasts in women is often quickly relieved by first cleansing with white of egg, carefully drying, and then powdering with oxide of zinc. This plan is, however, ill adapted when the affected parts are allowed to be in motion, as with intertrigo of the nates. In such cases the ordinary benzoated zinc ointment, with double the officinal quantity of zinc oxide so as to make it drier and firmer, or vaseline with zinc, are better applications: they should be spread upon thin rags and kept in place with a suspensory bandage between the thighs. When a similar condition arises from riding or rowing, the interval between the periods of irritation are longer, and it is possible, by scrupulous cleanliness and diligent powdering, to procure healing without altogether stopping the cause.

There are, however, some cases of eczema, especially, I think, in the young, and of the moister kinds, which resent every kind of medication, and can only be treated by the sedulous employment of the most soothing and indifferent applications. Among these, as I have already pointed out, water cannot be reckoned; it almost always does harm, and alkaline washes are worse. Thin size, cold cream or vaseline, I think, are the most likely to succeed in such cases. Glycerine of starch occasionally succeeds when everything else seems to fail, but it often proves extremely irritating, and on the whole is I believe less used than it was some years ago. Glycerine has the advantage of mixing freely with water and may thus be used as a vehicle of tannin or of borax to moist surfaces and mucous membranes; but the withdrawal of water from the surface appears in itself to be sometimes an irritant.

I am convinced that for the common "eczema solare" of Switzerland and of the sea glycerine is with most persons of little or no use, and is far better replaced by vaseline, which should be gently rubbed in before the face begins to sweat, and renewed from time to time while the exposure continues.

After the acute stage of an eczema has passed, and it is as a chronic inflammation that it usually comes before us, astringents are still indicated, but instead of soothing and protection some stimulation is necessary. This we obtain by adding a mercurial salt, and the ung. metallorum of our Guy's Pharmacopœia is one of the best combinations for treating impetigo and chronic eczema. The red oxide ointment, either alone or added to ung. zinci, is also very valuable, especially in the most chronic and indolent forms which approach ulceration. In obstinate eczema, especially where of small extent and moist, the application of liquor potassæ, as advised by Dr. McCall Anderson, is often an efficient and rapid means of cure. Acid nitrate of mercury is the best application for local deep fissures of inveterate eczema rimosum. In the very chronic eczema, which is dry and scaly without much active inflammation and itching, tar ointment, or our ung. liq. carbonis detergentis are indicated. For single patch eczema, if wet, liq. potassæ, if dry, ung. picis liquidæ is best.

Meantime we must prevent the irritation of scratching and rubbing by relieving its cause. Oxide of zinc, calamine, or borax, as ointments, weak corrosive sublimate wash or ung. hydrargyri ammoniati, hydrocyanic acid lotion (ʒ iv— ʒ vj to a pint), cyanide of potassium ointment (gr. ij to the ounce), and mere protection from the air by such indifferent applications as cold cream and vaseline:—these are all useful for the purpose indicated, the poisonous sedative being used with caution when the moist surface is extensive. One advantage of the continuous bath is the relief from itching it affords. But beside external remedies it is important, especially with children, to add sedatives to secure rest for the inflamed skin at night. Opiates are undesirable not only on general grounds for children, but also because they occasionally aggravate pruritus. Henbane is better, and combined with camphor as a pill or a draught often seems to suit old men better than any other hypnotic. Chloral hydrate is particularly adapted to children, and is best given to infants alone. With older children and adults, bromide of potassium and chloral hydrate make the best combination.

In obstinate cases of eczema of the hands the following method is almost always successful. Wash them thoroughly, removing all crusts, secretion, and dead epidermis, and cutting the nails short. Then rub vaseline gently in all over, put on a well fitting pair of kid gloves and keep them on night and day, only removing them for applying fresh vaseline.

But with all our care we find local remedies inadequate to the cure of perhaps half our cases of eczema. Even at Vienna medicines are taken internally in this disease.

Impetigo rarely needs physic, but when the child is pale steel certainly hastens the cure, beside doing good otherwise. Steel wine for infants, the saccharine carbonate of iron for older children; and the citrate of iron and quinine are all useful in treatment of children's eczema; but when they fail, and steel is still indicated, it is worth giving the tincture of the perchloride in glycerine and water before trying arsenic. In the severe form of chronic eczema in children, especially when not pustular and when widely distributed, arsenic is almost always necessary, and rarely fails of success. It must, of course, be begun in small doses, and must always be given with food, and then it scarcely ever disagrees. Children bear it very well even in full doses, and grow fat and rosy while taking it. I have given as much as fifteen drops of Fowler's solution three times a day to a child of seven years old without any but good effects. Sometimes the soda agrees better than the potash salt and it should always be well diluted with water. Except a little syrup to sweeten it no adjuvant is needed. Perseverance in this treatment is rewarded in the most inveterate cases. I have given the above in a case of universal eczema which existed from infancy till near puberty in a boy, and was cured at last in a few weeks by sedulous local treatment and persistent exhibition of arsenic notwithstanding sickness. I had equal success with a girl of sixteen, who had been subject to eczema from early childhood, and suffered terribly from its excessive irritability and its deformity. She could never go out without a thick veil, and every evening was obliged to retire to her bedroom on account of the itching, which then became intolerable. Under arsenical treatment the disease completely disappeared, and she is now able to go into society. This case was one of those which were once explained by the theory of "metastasis," for when the eczema was least troublesome the patient was subject to asthma, and this disappeared in spring and autumn, when the eczema became most severe. Since, however, the skin has become normal the cough and dyspnœa have also improved.

In some children with more or less obvious signs of tubercle in the lymph glands or elsewhere, the exhibition of cod-liver oil certainly appears to hasten the cure of eczema, as well as to improve their general health.

I have already referred to the importance of hypnotics in the treatment of irritable eczema in children.

In the acute weeping eczema of adults experience confirms the practice of giving saline laxatives along with local treatment. Friedrichshall or Carlsbad water, Epsom salts, and the "white mixture" of magnesia and sulphate of magnesia, or carbonate of soda with sulphate of soda and sulphate of magnesia, are the best kinds of purgatives. Occasionally soda and rhubarb succeed better. In the eczema of children few internal remedies are so useful as Gregory's powder. The value of mercury internally administered is almost confined to the disease in young children, and in some cases we cannot doubt its benefit. The carbonate of soda three parts with hyd. c. cretâ one part of our pharmacopœia is the best form in which to give it.

Iron is seldom needed unless obvious anæmia is present.

But with women, especially towards the menopause, sulphate of iron with sulphate of magnesia forms an excellent combination, especially if a few drops of dilute sulphuric acid be added.

In the irritable and obstinate eczema of elderly persons arsenic often appears to aggravate the malady. Local treatment assisted by internal sedatives is in these cases most useful, but occasionally purges appear to be of service, and certainly add to the physical and mental comfort of the patient. I believe that eczema occurs more often in connection with gout than does psoriasis or any other disease of the skin. In such cases colchicum is undoubtedly indicated.

The local persistent forms of eczema rarely benefit except by careful and persevering local treatment; but in the ordinary chronic dermatitis of the legs in elderly persons laxatives are no doubt a valuable adjunct to elevation of the limb, and artificial support of the enfeebled veins by flannel rollers, elastic stockings, or, best of all, by Martin's india-rubber bandage.

As to diet, we are for the most part content to follow the traditional warnings against salted food, spices, and preserves. I am sure that most children suffering from eczema benefit by a meat diet and some of them by the addition of stimulants. This also applies to anæmic adults, and especially to the case of women suffering from over-lactation or from menorrhagia. On the other hand, adults in general appear to benefit by taking less meat, no malt liquors, abundant diluents, and plenty of fruit and vegetables. In cases of the pruriginous eczema of the aged abstinence from fermented liquors is sometimes successful, though I have more than once known it fail.

The regulation of the diet of infants suffering from eczema is of paramount importance. We often find that before they are fully weaned infants are fed upon potatoes and other food containing indigestible cellulose or excess of starch. It is easy to see the mischief of this. But even when milk alone is given it often causes irritation, as shown by diarrhœa and vomiting; and if our local remedies are to succeed we must dilute it or mix it with lime water. Infants suffering from dermatitis, with consequent pyrexia, often "crave" for the breast or the bottle, not from hunger, but from thirst, and thus complete a vicious circle by overloading their stomachs with food, when, if they could express their wants, they would ask for water.

On the whole, internal treatment is most likely to be of value when used to help careful and energetical local treatment.—*Guy's Hospital Reports*, 1881, *p.* 224.

69.—ON THE TREATMENT OF ECZEMA BY BANTINGISM.

By BALMANNO SQUIRE, M.B.Lond.

It is familiar to every practitioner that eczema is specially common amongst infants, and particularly amongst lymphatic infants, that is to say, fat and pasty-looking infants. I do not refer only to those instances in which the very fatness of the infant is the mechanical cause of the complaint; that is to say, where a "fold" of skin in the fat infant becomes raw and discharging (intertrigo); but I refer to the well known fact that infants of this constitution are more liable than others to eczema (of the scalp and other parts), not coming under the head of "intertrigo," and that their eczema is more profuse in its discharge, whether that discharge be serous or purulent, than it is in other infants; it is also more obstinate.

I recently recorded in the Journal some experiments I had made with iodoform as an application in such cases, and which succeeded very well as a means of reducing the eczema of such infants from the discharging to the dry condition pretty rapidly. As to that, I was supported by other writers.

But from some observations I have recently made, I have reason to think that Bantingism as applied to such infants is as rapid in its effects, and if sustained, is of more permanent efficacy. Some years ago the late Mr. Banting was under my care for eczema, and I had an opportunity of conversing with him pretty frequently on the system of which he was the apostle. It is through this accident that the idea suggested itself to me.

I have used the word infant for the sake of convenience, but I refer rather to very young children. To Bantingise a suckling infant is, of course, not very practicable, but a child of two or three years old can be dieted very readily. It does not appear necessary to the end in view that the diet should be restricted in quantity as well as in quality, which of course was essential under the *régime* laid down by Mr. Banting, or rather by the late Mr. Harvey (the aurist) for him. It is simply necessary to limit the fat-producing elements of food for the amelioration of eczema in lymphatic young children. At least so I have found. By this means their excessive obesity becomes diminished and their eczema very remarkably improved within ten days of commencing the regimen, and that without any injury whatever to their general health, so far as I can judge.

In place of pure milk, they should take milk diluted with an equal or even a double quantity of water. In place of bread and butter they should take dry toast or dry biscuits; and with these particular articles of food they may be supplied indefinitely. All the fat is to be carefully cut away from such meat as they may partake of, and they should not be allowed pork, veal, or lamb. They may have poultry or game, or fish, except the oily kind of fishes, such as herrings, salmon, eels, etc.; and the fish they partake of should be broiled (not fried). They may eat boiled vegetable tops, but not vegetable roots, such as potato, parsnip, beet-root, turnip or carrot. Beef-tea (the melted fat being carefully skimmed off) is permissible in any quantity, and so also toast and water. Cooked fruit (not sweetened) may also be allowed.

But the principles of the Banting treatment are universally known. It only remains for me to say that cod-liver oil (the favourite remedy, *par excellence,* for the condition of which I am speaking) is quite incompatible with the treatment I am advocating. Of course, the health of the child should be watched; and it should be weighed at the commencement of its dieting, and afterwards from week to week.—*British Medical Journal, April* 8, 1882, *p.* 499.

70.—REMARKS ON BALD TINEA TONSURANS AND VEGETABLE PARASITES.

By Robert Liveing, M.D., F.R.C.P., &c.

On several occasions, I have called attention to the occurrence of smooth bald patches in tinea tonsurans, exactly resembling alopecia areata. I have also mentioned the fact that when, in these cases, one patch becomes quite bald, the others quickly follow suit. A remarkable example of this has lately come under my observation. Two brothers have been under my care for some time past with ordinary well marked tinea tonsurans, with abundance of the trichophyton. Their parents objected to vigorous treatment, and therefore mild remedies only have been used, which have not at any time produced the slightest soreness. The elder boy had six or seven separate spots of tinea tonsurans, and, during the last fortnight, every one of these spots, great and small, has become perfectly bald, smooth, and shining, with a well defined margin of healthy hair. What I wish specially to call attention to is, (1) the suddenness of the change, (2) that the bald spots are strictly confined to patches previously affected with tinea tonsurans, not a trace of baldness existing on any other part of the head. In this instance, there is not a stump nor a hair to be found, though, when I previously saw him, the patches were thickly covered with stumps and hairs. In other cases of this kind that I have met with, it has been generally possible to find one or two stumps left either on or at the margin of the patch, by the microscopic examination of which the nature of the case could be determined. When the baldness is as perfect as it is in this instance, we may probably regard it as a spontaneous cure. In the younger boy, the tinea tonsurans has run an ordinary course. It is somewhat difficult to say whether we should regard a case of this kind as one of true alopecia areata following on tinea tonsurans, or as a peculiar and exceptional feature in tinea tonsurans itself. Nor do I think the question of much importance, provided that we remember that infection is not always removed; or, in other words, that the denudation is not in every case so complete as to remove all the stumps affected with the *Trichophyton tonsurans*. A want of knowledge of this fact has, in past times, led to two mistakes: the one, that alopecia areata is a *parasitic* disease; and the other, that it is *occasionally contagious*.

It has been pointed out by several observers that, in almost all chronic scaly affections of the skin, one can, by a diligent examination of the scales under the microscope, find the spores of a vegetable parasite commonly called *Torula vulgaris*. I can quite confirm this fact from my own observations. I would remark, however, that those who are not aware of it are sometimes misled into the belief that they have discovered a parasitic disease, when, in truth, they have only found *Torula vulgaris*, which is quite harmless, and which is a consequence rather than a cause of the disease. An example of this lately came under my observation in the case of a lady about thirty years of age, who had suffered nearly all her life from a well known chronic folliculitis of the eyebrows and scalp depending on constitutional causes; the skin was scaly, and the hairs fell out before they had arrived at maturity. She was told by two medical men abroad, who had examined the scales and hair under the microscope, that the cause of the disease was a vegetable parasite; the result of the treatment was, however, not satisfactory. When she subsequently came under my notice, it was not very difficult to find here and there *Torula vulgaris* in the scales attached to the hairs, but the hairs themselves were quite free; the mistake was thus easily explained. I have met with several other examples of similar mistakes.

With regard to an affection of the skin which I have described as a form of circinate lichen, and which has been supposed by some observers to be pityriasis versicolor, and of course of parasitic origin, I would remark that a very careful series of observations, extending over several years, quite satisfied me that the two diseases are quite distinct. I am quite prepared to admit that, when pityriasis versicolor becomes a little inflamed, as it sometimes does in those who happen to have a tendency to eczema, it bears a close superficial resemblance in appearance to this variety of lichen circinatus; but the microscopical characters of the two are always quite distinct. As to the name circinate lichen, I lay no stress on it whatever; I adopted it to avoid the introduction of a new name; the disease has, however, since been described under a new name.

I hold that it is now quite out of our power to alter to any considerable extent our nomenclature of skin-diseases, however much we may object to some of the terms in use. Many objectionable synonyms will gradually drop out of use; and this is nearly all the improvement we can expect. The most vigorous efforts have been made to get rid of the name typhoid fever; but the word is still as much used as ever. The story of the emperor who failed in an attempt to change a quantity in a Latin word should not be forgotten by the inventors of new names for old diseases.—*Brit. Med. Jour., April* 8, 1882, *p.* 496.

71.—ON SODA AS A REMEDY FOR BURNS AND SCALDS.

By F. PEPPERCORNE, L.R.C.P., M.R.C.S.

Accidental burns and scalds, even when not very severe, extensive, or dangerous, commonly cause so much pain for an indefinite time, depending probably as to duration and severity a good deal on the age of the sufferer, and on the greater or less degree of sensitiveness of the individual's skin or constitution—not forgetting the feverish reaction, and the dangerous internal secondary inflammations that are apt to follow in some cases,—that any easily applied and quickly available remedy and relief, without perhaps the immediate necessity of calling in professional assistance, will be acknowledged as a boon by most persons; and especially so, when it is remembered that the sooner the agonising burning pain in the part can be allayed, the less chance there is of dangerous secondary effects, besides sloughing, &c., so severely trying to children and old persons.

The usual first applications to these painful injuries, whether so-called *popular* remedies, or such as are usually recommended by members of the profession, are numerous enough, but cannot unfortunately hitherto be considered as generally successful in giving certain and speedy relief from pain, and too often, intense suffering. One friend will recommend that the parts be covered with flour from the dredger; another will advise fine cotton-wool or wadding; another, starch in powder, or soap, or treacle, or the so-called Carron oil, &c.: but hardly one of such applications can be said to give more than very uncertain or temporary relief from pain; although perhaps, by occupying the attention of the sufferer, they may in this way prove of some *mental* benefit during his sufferings; being indeed employed really for want of anything better; although in fact, some of these applications, such as treacle, flour, starch, &c., prove so disagreeable in their after-effects, being often difficult to remove and renew, as to add frequently to the poor patient's depression and suffering, owing to their adhering to the injured parts in dry cakes very irritating to the raw surface.

It is now many years ago (see London Medical Gazette of March, 1844) that the author, whilst engaged in some investigations as to the qualities and effects of the alkalis in inflammations of the skin, &c., was fortunate enough to discover that a saline lotion, or *saturated* solution of the bi-carbonated soda in either plain water or camphorated water, if applied speedily, or as soon as possible, to a burned or scalded part, was most effectual in immediately relieving the acute burning pain; and when the burn was only superficial, or not severe, removing all pain in the course of a very short time; having also the very great advantage of cleanliness, and, if applied at once, of preventing the usual consequences—a painful blistering of the skin, separation of the epidermis, and perhaps more or less of suppuration.

For this purpose, all that is necessary is to cut a piece of lint, or old soft rag, or even thick blotting-paper, of a size sufficient to cover the burned or scalded parts, and to keep it constantly well wetted with the sodaic lotion so as to prevent its drying. By this means, it usually happens that all pain ceases in from a quarter to half an hour, or even in much less time.

Where the main part of a limb, such as the hand and fore arm or the foot and leg have been burned, it is best, when practicable, to plunge the part at once into a jug, or pail, or other convenient vessel filled with the soda lotion, and keep it there until the pain subsides: or the limb may be swathed or encircled with a surgeon's cotton bandage previously soaked in the *saturated* solution, and kept constantly wetted with it, the relief being usually immediate, provided the solution be saturated and cold.

What is now usually sold as bi-carbonate of soda is what I have commonly used and recommended; although this is well known to vary much in quality according to where it is manufactured,—but it will be found to answer the purpose, although probably Howard's is most to be depended on; the common carbonate being too caustic. It is believed that a large proportion of medical practitioners are still unaware of the remarkable qualities of this easily-applied remedy, which recommends itself for obvious reasons.—*Practitioner, Jan.* 1882, *p.* 15.

72.—ON BORACIC ACID AS AN ANTISEPTIC IN CERTAIN SKIN AFFECTIONS.

By George Thin, M.D.

The last few years has witnessed the introduction into general medical as well as more strictly surgical practice of a variety of antiseptic substances. In many instances the choice of an antiseptic is not a matter of much importance, but in others the employment of an odourless and non-irritating substance is of some consequence. In the latter respect I have found it convenient to use boracic acid in certain skin affections, and I propose to adduce some instances in illustration of its value.

In 1880 (see *Retrospect*, vol. 82, p. lxiii) I described a method of treatment by which persons suffering from that very great affliction, an offensive odour from the feet, can get rid at once and permanently of the smell. The treatment was based on the proof that the smell is caused by the development of a bacterium (*Bacterium fœtidum*) in the mixture of sweat and serosity with which stockings and boots are in such cases soaked. By wearing stockings and cork soles saturated with boracic acid the smell is at once annihilated.

I have reason to know that the method was found efficacious in the hands of others who had adopted it, but as its success cannot, I believe, be too widely known, I am induced to publish additional evidence of its value. The two following cases exemplify the rapidity of the cure.

A gentleman who consulted me by letter gave this account of his case :—My age is twenty-one, and for four or five years I have suffered very much from profuse perspiration of the feet. It continues, more or less, all the year round, being worst during summer. After taking exercise, on taking off my shoes (I find them cooler than boots) my socks are saturated with perspiration, more especially at the heels and toes, the hollow of the foot being nearly dry. On taking off my shoes at night, a very offensive odour emanates, my socks being as wet as if they had been taken out of water.

The smell and profuse perspiration persisted in spite of frequent changes of socks, and he had had to leave a situation in London and go home, for no other reason than this condition of his feet. During this time he had tried various treatment, such as salt water, alum, Fuller's earth, zinc ointment, hot and cold water baths, &c., in vain. I recommended him to try the boracic acid treatment. The result was a complete success. The smell was at once got rid of, and what it is of some interest to note, within a short time under the use of the acid the perspiration greatly abated.

The next instance is that of a servant girl in whose case "all sorts of things," amongst them alum, vinegar, salt in water, powdered chalk, charcoal sprinkled in the stockings, &c., had been tried without success, and it may be reported in the words of her employer, who at my request has sent me the following statement:—"A girl, aged fourteen years, entered our service in the spring of 1879. In the course of the following summer her feet perspired very profusely, and emitted a most disagreeable smell, which gradually became so offensive that no part of the house where the girl had occasion to be was free from it, and her fellow servant could not take her meals near her. After a course of treatment with boracic acid recommended by Dr. Thin, her feet have been completely cured, and now only occasionally, and by way of precaution in hot weather, she uses the acid to prevent the possibility of the offensive smell recurring."

That the disappearance of the smell should be coincident with the use of boracic acid is intelligible, and only what was to be expected. I anticipated the result before I verified it by experiment. That the perspiration should be lessened in quantity is however a very different thing, and the explanation is not on the surface. There seems little doubt of the fact. The patient in Case 1. specially noticed this effect of the treatment, and I had an opportunity of verifying a like result in another patient treated two years ago. In this patient, under the use of the acid the tendency to profuse perspiration gradually diminished, and has now completely disappeared. It would seem as if the application of a saturated aqueous solution of boracic acid to the epidermis had some effect on the amount of sweat poured out. I am inclined to suppose that this results from some change in the horny layer of the epidermis. In the case of the last patient to which I have just referred, the epidermis of the soles of the feet became harder than usual, and at some parts developed into a thick, horny crust. This condition of the epidermis, there is no doubt, was produced by the boracic acid solution.

There is a certain class of cases in which an ointment of boracic acid is exceedingly useful. The apprehensive watchfulness of a patient who was recently under my care has led to what I believe to be a great improvement in the preparation of this ointment. The young man to whom I have alluded was suffering from a scrofulous sore, following abscess, in a gland, and I prescribed an application of boracic ointment. He used several pots of ointment, and was dissatisfied at finding that the ointment supplied was not the homogeneous mass that he expected it to be. He chanced to send his prescriptions to be made up by Messrs. Savory and Moore, and I called on them in reference to the mode of preparation. They informed me that there were two formulæ for preparing the ointment, one given by Mr. Lister and one devised by themselves. Samples prepared by both formulæ had been supplied to my patient, and neither of them seemed to him or to me, nor indeed to Messrs. Savory and Moore, to be completely satisfactory. The effect of this was that those gentlemen were fortunately stimulated to consider whether a better ointment could not be prepared, a problem that was soon solved by Mr. Brownen, the analytical chemist of the firm, who in a few days submitted to me a cream-like ointment of perfect consistence. For various conditions of ulceration this ointment will be found of value. Messrs. Savory and Moore have been so good as favour me with the following communication regarding its preparation:—

"The peculiar physical condition of boric acid renders it extremely difficult to reduce the thin sharp-edged crystalline plates to such an impalpable powder as would be unobjectionable in an ointment. This difficulty led us to experiment with the solvents and solutions of boric acid, and we found that neither fatty substances or vaseline would dissolve a sufficiency as to render the combination of medical value. As glycerine is a good solvent of boric acid, and not so objectionable as alcohol, &c., we prepared a solution of boric acid in glycerine and incorporated it with a fatty basis of white wax and almond oil. This gave us a soft homogeneous cream, free from all irritating crystalline plates of crystal and quite permanent. Our experiments with vaseline were not so satisfactory, as the glycerine showed a tendency to separate from the mineral fat, and seemed not so ready of combination as with the wax and oil. The ointment is practically a glycerine cream, and resembles cold cream rather than an ointment. It might, however, be called 'Ointment' or 'Cream' of boric acid, and perfumed if thought necessary."

As an example of the efficacy of this ointment or cream in superficial affections produced by what may be loosely but sufficiently accurately for my purpose described as "bad action," I will give the history of an illustrative case. A young man, whilst on a walking tour, found that some blisters appeared on the dorsum of one foot, which he at first attributed to his boots being too large. In spite of care and ordinary precautions the excoriated surface became worse. When I saw him he was so lame that he could only walk with great difficulty. The foot was much swollen. The dorsum was red, tense, and painful, and covered with broken bullæ, the remains of epidermis on the raw surfaces being of a greenish-black colour. The pain extended up the whole limb, and a number of glands both above and below Poupart's ligament were swollen and painful. I prescribed perfect rest, and that the dorsum of the foot should be kept smeared thickly with boracic ointment. In twenty-four hours the œdema and erythema had much diminished, and the pain had almost entirely abated. The enlarged glands had ceased to be painful, and had diminished in size. The improvement went on so rapidly that within a few days he was able to come to my consulting-room and report himself almost well.

Six weeks afterwards a similar inflammation attacked the other foot, and as this time, the man being of sedentary habits, there was no question of the irritation being produced by the boot, it was attributed to a dye in the stocking. He had worn the same stockings when on the walking tour. They coloured water green, and this suggested to my patient that the stockings were dyed by some poisonous substance. He afterwards informed me that a free use of the same ointment and a couple of days' rest sufficed to effect a cure.—*Practitioner*, *Dec.* 1881, *p.* 401.

AFFECTIONS OF THE EYE AND EAR.

73.—PSEUDO-MEMBRANOUS (DIPHTHERITIC) CONJUNCTIVITIS—LOCAL APPLICATION OF SULPHATE OF QUININE.

By John Tweedy, F.R.C.S., Assist. Ophthalmic Surgeon to University College and to the Royal London Ophthalmic Hospitals.

Two years ago I recorded several cases of true diphtheritic ophthalmia that had been successfully treated, so far as concerned the integrity of the cornea, by local applications of sulphate of quinine (Lancet, 1880). Since then I have similarly treated four other cases, and though the cornea remained wholly intact in only one of these, they all recovered with useful sight, and with, at worst, but slight nebulæ. To the quinine I ascribe the credit of saving the cornea in these cases from total destruction. Three of the four cases occurred in men and one in a woman. Two began as purulent conjunctivitis of gonorrhœal origin, and two were pseudo-membranous from the first. The former, a man and a woman, were admitted with advanced purulent conjunctivitis of the right eye, the left being free. In both instances, within a few days of admission, and while the inflammation of the right conjunctiva was subsiding, the *left*, in spite of protection by Buller's shield, became the seat of adherent pseudo-membranes. In neither did much pseudo-membrane appear on the right conjunctiva. As regards the two cases that were primarily pseudo-membranous, it is noteworthy that both occurred in men engaged in handling animal carcasses, one being a porter in Smithfield meat-market, the other a slaughterman in the country. No direct contagion from pharyngeal diphtheria or other similar disease could be traced in either of the cases. The meat-carrier thought he had acquired his disease from contact with an acrid "sweat" on some diseased American pork he had been carrying. The slaughterman did not connect his illness with his occupation, though he volunteered the statement that he had often killed diseased animals, and that but few calves have perfectly healthy livers, "not one in ten," most of them containing larger or smaller collections of putrid "matter."

I need not relate the details of the cases; they were all treated in the wards of the Royal London Ophthalmic Hospital.

As soon as the nature of the disease was definitely recognised, all other treatment, if any, was stopped, and quinine lotion, containing four grains of sulphate of quinine, with a minimum of dilute sulphuric acid to an ounce of water, was alone employed. As far as possible the diseased surfaces were kept constantly bathed with the solution, the conjunctival sac being converted, as it were, into a trough holding the quinine lotion. A bowl of the solution was also placed within reach of the patient, who washed the eye frequently and kept a well-soaked compress constantly applied in the intervals. Besides these applications by the patient and by the nurse, the house-surgeon visited each case three or four times a day. On these occasions the lids were everted and the conjunctival sac thoroughly cleansed with the quinine lotion. The superficial disintegrated portions of the exudations were then gently removed with wet lint, care being taken not to aggravate the inflammation by rough handling or by rude attempts to tear off the pseudo-membrane. Usually, the quinine lotion was iced. In two cases the local application of powdered sulphate of quinine was tried at first, or sulphate of quinine rubbed up with an equal part of calomel; but, in addition to causing great pain, the powder did not appear to be so beneficial as the quinine in solution, and was therefore soon abandoned.

The virtues of the quinine are, I believe, specific in the diphtheritic exudation, and manifest themselves by checking or controlling the inflammatory process and the limiting the inflammatory neoplasms. Whatever may be the influence of the minute organisms that are said by some writers to be essential factors in producing diphtheria, it is certain that in diphtheritic ophthalmia the cornea is destroyed by the degree of the inflammation and the amount of concurrent exudation. The bacteria or vibrios or micrococci (for there is a hopeless disagreement as to which are the noxious organisms) do not destroy the cornea; nor, on the other hand, do strong applications of carbolic acid, which is, we are told, so fatal to "germs," arrest the progress of the pernicious pseudo-membrane in time to save the cornea. Three of the cases to which I have referred were at first treated by frequent ablutions with a five per cent. solution of carbolic acid, and in every instance the pseudo-membranes rapidly spread under these applications, whereas they were immediately controlled by the quinine lotion. Personally, I have a strong impression that the carbolic acid did harm, by aggravating the inflammatory process, and thereby increasing the amount of exudation. But, as formerly remarked, quinine not only checks the inflammation, but it is likewise detergent and antiseptic.

In my previous communications I stated briefly the reasons that led me to employ quinine in cases of true diphtheritic ophthalmia. With these reasons I am still content. Indeed I would now even emphasise them, as I would also emphasise the fundamental difference between the pellicular membrane of ordinary purulent ophthalmia and the parenchymatous pseudo-membrane of diphtheritic conjunctivitis. Extended experience has satisfied me that the two forms are essentially distinct and separate in their pathological as well as in their clinical relations. The pellicular form is amenable to simple treatment, and need not give much anxiety, whereas the pseudo-membranous is terrible in its ravages and appalling in its possibilities. Not a little harm may be done by confounding these two diseases. An application of nitrate of silver to the pseudo-membranes of diphtheritic ophthalmia increases the damage, whereas judicious applications speedily cure the membranous variety of purulent ophthalmia. The difference is appreciable from the earliest to the latest stages. Even when the pseudo-membrane is thin and detachable, its removal does not expose a swollen and vascular mucosa, but a smooth, pale surface often of a dull leaden hue. Later on the difference is still more marked; the membranous form only affects the epithelial layers of the mucous membrane, whereas the pseudo-membranes always invade the deeper texture, and heal by cicatrisation. In all the cases I have diagnosed as diphtheritic conjunctivitis cicatrisation was always pronounced.

It has by some been objected that the term *diphtheritic* conjunctivitis is a misnomer because the disease is not usually attended by grave constitutional symptoms, and because the pseudo-membranes do not spread to other parts. To these criticisms I would answer, first, that even pharyngeal diphtheria is often unattended by much constitutional disturbance; and, second, that the conditions of the conjunctival mucous membrane are little favourable to the extension of the disease down the nasal duct. There are many reasons for believing that diphtheria is primarily a local disease, and that the constitutional infection occurs secondarily. The poisoning in pharyngeal diphtheria is due partly to the inspiration of the noxious gases generated in the putrefying pseudo-membrane, and partly to the ingestion of the detritus from these decayed masses. In the case of diphtheritic ophthalmia, on the contrary, the disease is shut out from the system by the conjunctiva and underlying tissues, and all the discharge escapes externally, so that the risks of constitutional infection are few, though not altogether absent. Of the nine cases that I have recognised as genuine diphtheritic conjunctivitis two (infants) died within three weeks, one of what was called "inflammation of the lungs," the other of pharyngeal diphtheria. Five had more or less extensive adherent pseudo-membranes inside the mouth or in the pharynx; and most of the patients showed extreme, almost characteristic, pallor, accompanied with general malaise. As regards the extension of the pseudo-membranes from the conjunctiva down the nasal duct, it may be said that if the diphtheritic pseudo-membrane rarely spreads from the nasal cavity up the nasal duct into the conjunctival sac, it is not surprising that it should so seldom travel in the opposite direction. The canaliculi and the nasal ducts may roughly be compared to funnels with the mouths downwards; hence it is easier for inflammation to extend from the nasal cavity to the conjunctiva than from the conjunctiva to the nose. The canaliculi are normally too small to allow the pseudo-membrane to pass through them, and very little swelling of the mucous membrane would obliterate them altogether. It does, nevertheless, sometimes happen, as I have just hinted, that even when the diphtheritic process begins in the eye it shows itself subsequently in other parts. This actually occurred in the meat-carrier referred to above. Four days after his admission into the hospital, and after the conjunctival mucous membranes showed appreciable improvement, he complained of soreness of the throat. Within twenty-four hours his tongue, roof of the mouth, and inside of the cheeks and lips were covered with an adherent dirty coating, which could not be rubbed or pulled off. The pharyngeal mucous membrane was red and swollen, and soon become completely covered with a thick pseudo-membrane, exactly resembling that on the conjunctiva. He eventually made a good recovery, and left the hospital with perfectly clear corneæ, though his palpebral conjunctivæ were contracted by dense cicatrices. Furthermore, it should be borne in mind that the pseudo-membrane of diphtheria usually spreads more quickly by contiguity of tissue than by continuity. This is especially noticeable in pharyngeal diphtheritis where, say, one tonsil becomes affected, then the contiguous portion of the uvula, and then the intervening arch. So in the conjunctiva two opposing portions of ocular and palpebral conjunctiva become affected long before the pseudo-membrane has time to travel round by the oculo-palpebral fold.

One word of explanation may be necessary—namely, that all four cases were of a very severe type. In one case (gonorrhœal) the conjunctival sac looked as if it had been filled with dirty tallow, except opposite the cornea, which was of a turbid sea-green colour. In the case where the pseudo-membrane was least in amount and extent (the slaughterman) the cornea suffered most. After the pseudo-membrane had disappeared a superficial ulceration spread over the greater part of the cornea, but did not cause much opacity.—*Lancet, Jan.* 7, 1882, *p.* 6.

74.—ON THE USE OF SULPHIDE OF CALCIUM IN STRUMOUS OPHTHALMIA.

By Simeon Snell, Esq., Ophthalmic Surgeon to the Sheffield General Infirmary and to the Institution for the Blind.

The good effects resulting from the use of sulphide of calcium in the sores of scrofulous children, and in other affections associated with this diathesis, have been particularly insisted upon by Ringer. It is now my purpose, however, to speak of its value in "strumous ophthalmia." Under this head I allude to phlyctenular and pustular conjunctivitis and keratitis, the characteristics of which I need not further mention. In many of these cases, when other remedies have been used for some time with little or no benefit, the sulphide of calcium has proved of great service. I do not know if it has been much, if at all, recommended in this class of cases, but I was led in the outset to employ it by noticing its beneficial effects in other scrofulous affections, and this especially in the practice of my friend Dr. Dyson.

The sulphide will be found particularly serviceable in those cases of children with manifest strumous habit, enlarged cervical glands, swollen face, the eyelids tightly closed, photophobia, and where, on opening the eyes, a gush of hot tears is emitted, and examination of the ocular surface discloses one or more phlyctenules on the cornea, or it may be merely increased vascularity of conjunctiva. These cases treated by the ordinary constitutional and local remedies are often tedious, but with the sulphide of calcium, coupled with the usual applications to the eyes, such as atropine and warm fomentations of poppy, or what not, frequently quickly yield a happy result. In other cases also of phlyctenular conjunctivitis or keratitis, and not alone in children, the good effects of this medicine are conspicuous. Of course, like all other drugs, it will be hardly likely to be suitable for, or to benefit, all cases, but I have now employed it with good results so frequently that I am quite satisfied as to its being a useful remedy. After little or no benefit with steel in its various forms, and cod-liver oil, the rapid recovery often after the substitution of the sulphide has been astonishing. The mode of administration is generally in the form of a powder, and from gr. $\frac{1}{10}$ to gr. $\frac{1}{4}$ of the sulphide, with a few grains of sugar of milk, are given about three times daily. In this way children take it readily. The following are brief notes of some out of several cases:—

Case 1.—Annie B., aged 7, from Hoyland, near Bemsby, came to the Sheffield General Infirmary on Feb. 4, 1881, as an out-patient, suffering from phlyctenular keratitis, especially in the left eye. She improved a little under treatment, and then became worse again. At different times the following remedies were prescribed internally: Syr. ferri phosp. co.; cod-liver oil; hyd. c. creta, &c., &c.; and locally, in the course of treatment, atropine, eserine, warm fomentations of various kinds; a seton was placed in the temple, and the ulcer in the left eye was also touched with nitrate of silver. July 22. All other treatment abandoned, and sulphide of calcium (gr. $\frac{1}{10}$) prescribed. July 26. Better. Ulcer in left eye nearly healed. Aug. 2. Quite well. A few days later discharged cured.

Case 2.—Ellen S., aged 11. Came to infirmary April 12, suffering from enlarged cervical glands and phlyctenular keratitis in both eyes, and photophobia, especially left side. Treatment: atropine, hyd. c. creta powders; and later on, cod-liver oil, syr. ferri phosp. co., and eserine substituted for atropine locally, and poppy fomentations also were used. May 3. The glands on the left side seem disposed to suppurate. Eyes no better. Ulcer in upper part of left cornea apparently inclined to perforate. Directed to continue ol. morrh. eserine, and syr. ferr. co., and sulphide of calcium—gr. $\frac{1}{8}$ prescribed three times a day. Immediate improvement; the cervical glands suppurated, but in two or three weeks the eyes were well.

Case 3.—Emma O., aged 12. Phlyct. keratitis in both eyes. Admitted at infirmary April 15. Little benefited under ordinary treatment. Aug. 2. Ordered calc. sulphid. gr. $\frac{1}{10}$, all other treatment, local and general, to be stopped. Aug. 16. Much better. Aug. 30. Discharged cured.

Case 4.—Robert G., aged 11. Phlyctenules on both corneæ; came under observation Oct. 21. Treatment adopted: hyd. c. creta powders, atropine, and tinct. iodi. applied once or twice to lids. Nov. 22. No better. Great intolerance of light. Calc. sulph., gr. $\frac{1}{8}$ t. d.; to continue atropine: Nov. 25. Better. Dec. 2. Nearly well.

Case 5.—A young lady, a medical man's daughter, from a distance, at present under treatment, appears in like manner to be benefiting from the use of the sulphide of calcium. There was great intolerance of light. Phlyctenular keratitis; one cornea being rendered much opaque from previous attacks. She had been some time ill, and other remedies had been adopted. She had enlarged cervical glands, and came of a strumous family.—*Practitioner, Jan.* 1882, *p.* 17.

SYPHILITIC AFFECTIONS.

75.—ON THE NON-SPECIFIC TREATMENT OF SYPHILIS.

By Francis Cadell, F.R.C.S.Ed.

When beginning practice in Edinburgh, now nearly twelve years ago, I was led to discard mercury in the treatment of syphilis from the experience I obtained of the non-mercurial treatment while watching the practice of Professor Boeck and others who did not believe in the specific action of mercury. Since then I have steadily practised the non-specific treatment of syphilis, and, by watching the course of many cases for a length of time, have come to the conclusion that it is the best method, and the one least likely to be followed by gummatous complications. Believing that the anæmic state of the blood and the depressed condition of the vital powers (which certainly occur during the course of syphilis) can be quite safely combated without mercury, I have trusted entirely, in the treatment of this disease, to the blood-forming and tonic medicines which are found useful in similar conditions of the body in other diseases. I shall now briefly describe the the treatment adopted.

Syphilitic Chancre.—Whatever be the amount of induration or ulceration connected with this sore, the treatment of it should be conducted on ordinary surgical principles. No mercury is required either internally or externally, and the sore will be found to behave very much the same as other ulcers do under similar conditions when submitted to rational treatment. Caustics are rarely necessary, and should only be applied when they will assist the process of healing, and not under the expectation that even in the earliest stage of the chancre they may ward off constitutional affection. I hold with Ricord that the appearance of the chancre is the first indication that the blood is contaminated by the syphilitic virus. When it is considered necessary to apply nitric acid for surgical reasons, a plan has been suggested by Dr. Keyes of placing first a drop of pure carbolic acid on the sore, and then drying the surface with blotting paper, before applying the stronger acid, the well-known anæsthetic properties of carbolic acid making the subsequent application less painful. There is an attempt at present to resuscitate the treatment by excision of the chancre, in order to prevent constitutional infection. This practice has not been found successful, for reasons stated above when speaking of cauterization. Water dressing or any of the ordinary astringent lotions may be applied according to indications. Boracic ointment and iodoform are also useful applications, more especially the latter. It is best to apply iodoform in powder, and if it were not for its persistent smell it would be very generally applicable. The addition of tannin has been recommended to lessen the smell of iodoform, but it makes little difference. By these simple means the worst forms of chancre will heal, although it may be necessary also to give tonics to improve the condition of the patient, when that is required.

In the lymphatic engorgement which accompanies the syphilitic chancre, suppuration is rare, and even when it seems most imminent it is well to refrain from using the knife, as it is often astonishing how resolution may take place even when fluctuation is seemingly distinctly felt.

In a case of indurated chancre with multiple adenopathy it is necessary at once to consider how best to prepare the patient to resist the depressing effects of advancing constitutional syphilis. He must be warned that in order to get well over this tedious and often severe illness, a high state of bodily health has to be maintained, the most scrupulous cleanliness practised, to keep regular hours, eat plainly, take stimulants in very moderate quantities and only with his meals, and to give up the use of tobacco absolutely in all its forms. The state of the mouth should be carefully attended to, in order that no local source of irritation to the mucous membrane may be allowed to exist. Among some of the first symptoms of constitutional syphilis, appearing often before any skin eruption, are periosteal swellings and bone pains of a more or less transient character, the pains being worst at night, and having a tendency to subside on the development of the skin eruption. For these symptoms iodide of potass in gr. x. doses is useful, but only as directed against the bone and periosteal symptoms, and not as having any special reference to this the so-called secondary stage of syphilis. I believe iodide of potass to be practically useless for the relief of any symptoms occurring during the secondary stage, except those above referred to.

After these preliminary symptoms, which are not constant, come the eruptions of the skin and mucous membrane, falling off of the hair, affections of the nails, eye, &c.

There is now invariably a tendency to anæmia, with depression of vital power, and to obviate these conditions is the chief aim of treatment.

At this stage, and often before, mercurialists begin their specific treatment, and the most eminent advocates of the method recommend that it should be continued without intermission for two years at least. It may be of interest to give here shortly the views of Bärensprung on the advantages of the non-mercurial over the mercurial treatment. Although formerly a strong mercurialist, he raised his voice in Berlin against the system. He believed that mercury caused the secondary manifestations to disappear, but did not hinder the further development of the syphilitic process. The outbreak of new symptoms is hindered, and in this way the course of the disease is prolonged. Bärensprung further asserts that, syphilis being a deep-seated disease of the whole organism, cases of rapid recovery under a few weeks are of rare occurrence, whatever the treatment may be. As a rule, recovery is very gradual, and may follow either mercurial or non-mercurial treatment. The advantage secured by the first is the rapid disappearance of the several symptoms of the disease; but this is more than counterbalanced by a double disadvantage, for, as mercury has an undermining effect on the entire constitution, the development of local disorganisations is favoured; and as the disease often for months and years remains latent, the ultimate recovery is protracted. The difficulty of the non-mercurial treatment is increased, especially at the commencement of the disease, by the severe and diffuse manifestations of symptoms not being hindered, and these symptoms being only slowly made to disappear; but, in return, it holds out the great advantage that the disease never remains concealed when it is not cured, and the ultimate recovery is at any rate advanced. This is a fair statement of the position of the non-mercurialists. Any influence mercury may possess over the superficial symptoms of early syphilis is, I believe, entirely due to its local action as a skin stimulant, whether it be given internally or applied externally, and that it has no specific action on the poison itself.

The treatment I find very successful in secondary syphilis is by supporting the general health, administering tonics, especially iron, when required, and insisting on careful attention to the hygienic rules already laid down. By these means a gradual and certain recovery will be brought about, not by any empirical nostrum or specific, but by the ordinary rules of rational medicine, which might be employed equally well in the treatment of similar conditions occurring in any other disease.

Local treatment is useful for the sore throat and mucous patches occurring within the mouth during this stage of the disease. Astringent gargles, with occasional touching of the patches with nitrate of silver, assists the healing. The moist papules or condylomata found between two skin surfaces and in the neighbourhood of the vagina and rectum merely require cleanliness and dryness for their removal. Dryness may be secured by the careful dusting of the part with fuller's earth, kaolin, starch, or such like substance.

In tertiary syphilis, or the stage of gummatous deposits, iodide of potass is found to be of immense benefit. It may be combined with tonics, carbonate of ammonia, and cod liver oil, when these remedies are indicated by the state of the patient. Gummatous tumours are treated as follows:—*1st*, What ought not to be done. Do not remove gummata either by the knife or caustic. Abstain from opening a gumma even when fluctuation is present, as resolution may occur at any stage of its progress. Examples abound where gummata on the point of opening have been seen to diminish and become absorbed under iodide of potass. *2nd*, What is to be done. Give iodide of potass in large and increasing doses, beginning with gr. x. thrice daily. The production of gummata is often combined with a low state of the organism, either anterior or foreign to the diathesis, or engendered by it, for which tonics and cod liver oil should be administered. In the early stages blistering is found to be a useful adjuvant to the internal treatment. After gummata have opened and formed ulcers, ordinary surgical principles must be our guide in treatment.

The same lines of treatment are to be followed in congenital syphilis.—*Edinburgh Medical Journal, Dec.* 1881, *p.* 496.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

76.—ON SOME POINTS CONNECTED WITH THE MANAGEMENT OF LABOUR.

By John S. Main, M.D., Manchester.

In the consideration of labour, it is well, I think, to regard it as a normal physiological process, in which nature is the active agent and ourselves the mere onlookers. In other words, that our interference is only required when nature—for some reason or other—fails to accomplish her usual amount of work; and that then our interference is based on the lessons we have formerly derived from her teaching. For the art of obstetrics (like some others) is not only an art "which shares with great creating nature," but also one

> "Which does mend nature—change it rather—
> But the art itself is nature."

Based on this principle, our services during the progress of labour are often invaluable.

In the first place, and I intentionally give it the preference, as being a point often overlooked, I would mention the importance of keeping up the power of the system. This, as I have already said, I believe to be a point often overlooked; and perhaps more so than would be the case were it not that our patients themselves don't always see the advisability of attending to it. Perhaps in no other physiological process is so much nerve energy lost as during labour, and when we bear in mind that this is continuous for from six to eight, or even more, hours, according to the case, certainly something must be done to make up for this drain on the nervous system. If this be neglected, what follows is, that as time wears on the woman's strength wears out. Further on—for want of replenishing—the lamp of her nervous energy begins to wane, then flickers, and finally goes out altogether, leaving her entirely helpless, and necessitating *instrumental* interference—in short, leaving the whole muscular system powerless, and the uterus in a state of *inertia*.

This is a state of matters well known to us all, and moreover, a state of matters which, in spite of every attention on our part, will in some cases follow. Still, this is certainly the exception, and much may be done in the way of avoiding it.

With this object in view, I have tried several methods. Amongst others, the administration of strong infusions of tea or coffee, of beef tea, of a combination of rum or brandy, and milk. None of these methods, however, did I find exactly suitable. The first I found certainly to be stimulating, but not affording sufficient nutriment; the second often caused sickness; and the beef tea could not be taken, besides not being often procurable; the third, on account of its containing alcohol, I soon discontinued, as I thought on several occasions harm had resulted from its use.

Lately I have been in the habit of using equal parts of milk and strong infusions of tea or coffee, and with unvaried success. This combination supplies both stimulation and nutriment in a simple form, is always at hand, and is, moreover, a mixture which few patients object to. I leave it to be tea or coffee, according to the preference of the patient, though I am inclined to think that of the two the coffee is the most beneficial. This I give in quantities of not more than half an ordinary teacupful at a time, pretty warm, and without sugar, during the continuance of the first two stages of labour. Stimulation, also, by means of words—encouragement I mean—must not be forgotten, especially in dealing with first labours, as, naturally, these patients tend to become fretful and discouraged. At all times, and in all diseases, hope is a good stimulant; and especially is it so here. On this point, Coleridge has well remarked, "he is the best physician who is the best inspirer of *hope*."

During the first stage of labour, a difficulty often met with is a rigid condition of the os uteri. To relieve this, I know of nothing better than a full dose of hydrate of chloral—30 grains. This sometimes puts the patient to sleep; but whether or not, it almost invariably. in the course of an hour or two, brings about relaxation. Another very effectual plan is to allow a stream of rather hot water to play against the os uteri, by means of a vaginal douche. Ergot in these cases certainly does harm, and this, I think, cannot be put in too strong terms.

Another difficulty sometimes arises, from lateral or forward displacement of the uterus. The latter, of course—pendulous uterus—only occurring in pluriparæ, and both these conditions, hindering or retarding labour, by the force of the uterine contractions being directed at a disadvantage. Where the displacement is lateral, perhaps the best help is got from the hands, placed one on each side of the uterus during contraction, so as to bring it into the middle line; where forwards, by keeping the patient on her back, or putting on a binder. Other difficulties and their management are too well known to need even referring to here. At the termination of the second stage, where there is much delay in the revolution of the head round the os pubis, threatening instrumental interference, I have found much assistance from the following manœuvre, which I have not found described in books. It is this—During the pains I get the forefinger of my right hand inserted between the head and the os pubis, and hooked round the protuberance of the occiput. Having done this, I make traction with this forefinger, and the occipital protuberance is generally prominent enough to allow of this; while, with the forefinger of the left hand, I make pressure well forwards in the central line of the head, so as to increase the flexion of the chin on the chest. With the aid of this manœuvre, I have succeeded in getting the head born in the course of a few pains, after the occiput had reached the os pubis. This is, in my opinion, a method much to be recommended also in all cases where there is a tendency to rupture of the perineum, as by increasing the amount of flexure of the chin on the chest, and lowering the occiput it takes the pressure considerably off that part. On the birth of the child, and the removal of the placenta, it is well to give in all cases, by way of safety from hemorrhage, a full dose of ergot, and to put on the binder. Strict quietness should also be enjoined, the room darkened, and the child put to the breast.

For the relief of after-pains—often so troublesome to primiparæ—I have found nothing so beneficial as the "red mixture" recommended by Dr. Graily Hewitt, consisting of tincture of lavender, spirit of sulphuric æther (Lond. P.), spirit of chloroform, and camphor water.

Lastly, as it is a well known fact that sore nipples not only interfere with the proper emptying of the breasts, but also tend by sympathy to lead to inflammation and abscess, besides being excessively annoying and painful to the mother, it is well that this condition should be put right at once if it exists. For this purpose, I know of nothing more serviceable than the lotion recommended by Dr. Playfair, in his *Treatise on Midwifery*, of which this is the formula:—℞. Acidi sulphurosi, ℥ss; gly. (c) acidi tannici, ℥ss; aq. ad. ℥ij. M. Sig. The lotion for the nipples.—*Glasgow Medical Journal, April*, 1882, *p.* 258.

77.—HOUR-GLASS CONTRACTION OF THE UTERUS TREATED WITH NITRITE OF AMYL.

By Fancourt Barnes, M.D., M.R.C.P.

I was called, at ten o'clock in the morning on Feb. 28th last, by one of the midwives of the Royal Maternity Charity, to a patient with retained placenta. On my arrival, I found that the patient, a secundipara, aged 22, had been delivered naturally at three o'clock in the morning of a living female child. The midwife stated that she sent for me, because she had been unable to deliver the placenta. On examination, I found that the umbilical cord had been separated from the placenta. The external os uteri was quite dilated, as was the cervical cavity; but the os internum and the circle of muscular fibres above it, called Bandl's ring, the chief seat of hour-glass contraction, were firmly contracted, and only admitted a finger, by which the placenta could be felt in the uterus. I now learn that the midwife, hoping to accelerate the third stage of labour, had given the patient a dose of ergot as soon as the child was born. I found it impossible to get my hand into the uterus to deliver the placenta. Bearing in mind the remarkable power which nitrite of amyl possesses in relaxing tension in the blood-vessels, I determined to test its action on the uterine spasm. The patient had three drops of the nitrite of amyl given her on a handkerchief to inhale, by Mr. Lingard. During the inhalation, the ring of muscular fibres round the os internum, which had been so rigid as to be absolutely undilatable, steadily yielded, until I could pass the whole hand into the uterus and detach the placenta, which was universally adherent. There was no hemorrhage whatever, and the placenta itself presented a remarkably exsanguine appearance. On referring to the third edition of my father's work on *Obstetric Operations*, I found the following. "We possess in ergot a great, a dangerous power of augmenting the force of the uterus. We want an agent endowed with the opposite effect, that will control and suppress uterine action. I consulted Dr. Richardson on this point. He tells me the desired power exists in the nitrite of amyl. Three minims of this added to one drachm of ether taken by inhalation is the form he recommends. It does not produce unconsciousness; but it is an anæsthetic as well as a sedative of muscular action. It is the antidote or opposite force to ergot. In it we have the desiderated 'epechontocic agent'." In the case in question, the drug certainly acted admirably. It relaxed the irregular contraction of the uterus, and acted as a sedative and anæsthetic without producing unconsciousness. The case is also instructive as an example of the dangers which may result from the administration of ergot before the expulsion of the placenta. The tetanic action was no doubt increased by the traction which had been made on the cord. It is well known that ergot, when given before the birth of the child, may cause its death. I believe this results from the blood being squeezed out of the placenta by the uterus. Although in cases of irregular contraction of the uterus that organ is firmly contracted, the contraction does not separate the placenta. On the contrary, in the cases I have seen, the placenta has been firmly adherent, as it was in this case. I am not aware that nitrite of amyl has been used to relax uterine spasm before. In it we possess, I think, a new and trustworthy addition to the resources at command for overcoming spasmodic or trismic contractions, which will not always yield to other remedies.—*British Medical Journal, March* 18, 1882, *p.* 377.

78.—PERINEAL CURVE AXIS-TRACTION FORCEPS.

By S. MACVIE, M.B., C.M., Chirnside, N.B.

Recent attempts to obtain the advantages of axis-traction in forceps operations have resulted in contrivances so cumbrous as little likely to be adopted by the main body of the profession. I am, therefore, induced to describe a modification of the double curved forceps for the attainment of the same object, which I have lately successfully employed, and which has the merit of simplicity to recommend it. This modification does away with the necessity for traction-rods and all other extra aids; and consists simply in the addition of a perineal curve to the ordinary double curved forceps.

Taking Simpson's "intermediate" forceps to work upon, I have added about two inches to the length of the stem of the blades, that is, of the part intervening between the commencement of the cranial curve and the lock. This additional length I have so bent that, with the blade *in situ*, it shall curve round the fourchette and pass backwards over the perineum until it touches a line prolonged downwards through the traction axis of the blades. At this point I have again bent the stem, so that the remaining part of the added length and the handle thereon shall continue in the traction axis line.

Such is the modification. Briefly, I have neutralised the pelvic curve by the addition of a perineal curve, so that the focus of resistance lies in the line of traction; and the instrument, in this respect, becomes practically a straight forceps. Nor is this advantage obtained at any cost, except the addition of one-and-a-half inches to the total length of the instrument. The "perineal curve" simply re-confers the power of axis traction which was sacrificed by the adoption of the pelvic curve, and without limiting the range of applicability bestowed by the latter. Therefore in the ordinary "intermediate" forceps, thus modified, we have an instrument of the widest utility, endowed with the power of axis traction; suitable wherever forceps are required, except where the obstruction is exceedingly high; and neither unhandy in the simplest nor inefficient in the most difficult cases. This modification is also valuable since it is equally applicable to the "long" forceps, and, moreover, it deprives the short straight forceps of the single superiority it possesses.

I do not pause to contrast in detail the merits of the "perineal curve" with other contrivances for the same purpose. In principle they are identical. But in efficiency the former is fully equal to the latter; while for handiness and simplicity it is much superior. But I may mention a few particulars in which my experience shows that it excels the instrument out of which it has grown.

1. With the tip of the blade directly in the line of the handle, its introduction is easier and its adaptation more precise.

2. When the under (posterior) blade is introduced first, the handle lays far behind the fourchette, and entirely out of the way of the introduction of the upper blade.

3. For the same reason, the operator is less interfered with by the patient's thighs, both while introducing the blades, and during the extraction of the head; and suffers less loss of power from the handles projecting far in front when the head is passing under the pubic arch. In fact, from first to last, the operator occupies a more commanding position.

4. The lock, being well behind the fourchette, cannot possibly inflict any injury upon the soft parts during the adjustment of the handles.

5. The instrument, being practically straight, is capable, when the head has fully entered the pelvis, of aiding the movement of rotation. This is an advantage afforded by no other instrument whatever at such an early stage.

6. Lastly, and mainly, it confers all the advantages of axis-traction. These need not be enumerated. But it may be permitted to say, that it does so with the slightest departure from the simplicity of the instruments in common use; and with no departure whatever from the manipulative procedure at present practised.

Messrs. Young and Sons, North Bridge, Edinburgh, are the makers.—*British Medical Journal, Jan.* 7, 1882, *p.* 9.

79.—ON PUERPERAL SEPTICÆMIA AND SEWER-GAS.

By John C. Ferrier, M.D.Lond.

In the British Medical Journal, for March 10th, Dr. Playfair refers, in his "Note on the Sanitary Condition of Bagshot Park," to the causation of puerperal septicæmia by sewer-gas. The following cases seem interesting, as showing the connection.

Case 1.—Mrs. S., a multipara, was confined on the morning of Feb. 2, 1881. Labour was normal; the child was at full time, and living. A few hours later I found her feverish, but the temperature was not noted. The next day she had headache and slight abdominal pains; temperature 102·2° Fahr. She continued ill for more than five weeks (until March 10th), with symptoms of blood-poisoning. The temperature was sometimes very high—over 105° on one occasion. After the illness had lasted a few days, she had pleurisy on the left side; and, towards the end of the attack, abscesses formed in the axilla, groin, labia, and gluteal region. There was no sore-throat and no pelvic inflammation. The lochia were not at any time very offensive. There were only slight rigors, and milk was secreted for some days. She recovered. Two or three weeks before her confinement, the water-pipes being frozen, the trap of the water-closet became blocked; and Mrs. S., while directing the removal of the obstruction, got a strong whiff of sewer-gas, and exclaimed at the time that she was afraid it would give her diphtheria. She was not seen medically before her confinement; but her husband said she had been feverish and ill for a week before it took place, and that she "felt so hot in bed that he was obliged to move away from her." There was no history of exposure to any other poison than sewer-gas. In the same house, two years before, several of her children had diphtheria from defective drains; but these were put to rights, and there was no further trouble until the water-supply was cut off by the frost.

Case 2.—Mrs. H., a multipara, was confined prematurely, at seven months, on July 22, 1881. The child lived one day, and died cyanosed. She was ill for several days before confinement, with pyrexia and abdominal pains. There was no local trouble to account for it. The day before confinement her temperature was 103°. Afterwards, she was better and less feverish for a day or two; but the temperature was never normal. She soon became much worse, and had severe rigors nearly every day; one wrist-joint became red and swollen, also the first metatarso-phalangeal joint of one foot. There was no throat trouble; the lochia were not offensive. Milk was secreted for some time, and required belladonna to stop it. She died on August 25th, one month and six days after confinement. There were not any cases of infectious disease in the house, nor could any fault be found with the sanitary arrangements. But, a short time before her illness, Mrs. H. had noticed a strong smell from a sewer, while she was out walking.

In both cases the illness commenced before confinement; local mischief only showed itself after the pyrexia had gone on for some days; and there was nothing in the history, symptoms, or progress of either case to lead me to suspect that any other poison than sewer-gas had been at work.—*British Medical Journal, April* 8, 1882, *p*. 497.

80.—ON IMPERFECT INVOLUTION OF THE UTERUS AND ITS CONSEQUENCES.

By Lombe Atthill, M.D., F.K.Q.C.P.I., Dublin.

Subinvolution of the uterus is the primary cause of very many of the ailments from which women suffer. In the first instance, it is frequently the cause of profuse menstruation; that this should be so is easily understood, for not only is the quantity of blood supplied to the uterus unduly great when this condition exists, but also the mucous membrane lining its cavity frequently becomes unhealthy; then it is the most common of all the causes that produce retroflexion of the uterus, and, at a later date, it predisposes to, if it be not the actual cause of, chronic metritis and hypertrophy of the uterus, while it markedly favours the occurrence of granular erosion of the cervix and of endometritis. I must, therefore, dwell at some length on so important a subject.

When we speak of subinvolution of the uterus, we mean that the process by which the womb regains its original size subsequent to delivery, or abortion, has been from some cause retarded or arrested; this process has been termed involution, and when it is incomplete we talk of the uterus as being in a condition of imperfect involution, or more commonly, of subinvolution.

The involution of the uterus should be completed within a few weeks after the date of delivery. It is one of the most remarkable phenomena which occur in the human body. The uterus, immediately before the expulsion of the fœtus, measures about fourteen inches in length, and weighs twenty-five ounces, often, indeed, even more. Immediately after, its size is diminished to considerably less than one-half its former bulk, its weight being proportionally reduced; while, if the process proceed regularly and unchecked by any cause, the womb will, after the lapse of five or six weeks, be less than three inches in length, and weigh but two ounces. The first step in this process is, that the supply of blood to the uterus is checked, and the circulation of blood through that organ interrupted, by the contraction of the muscular fibres of the uterus, a process which commences the moment labour terminates, and goes on in a more or less painless manner for some days subsequently; while, at the same time fatty degeneration and disintegration of tissue, on the one hand, and absorption on the other, rapidly complete the work of reducing the uterus to its normal size, and restoring its compactness of tissue.

But you can easily understand that numerous causes may interrupt this process; thus, in weakly, debilitated women, the uterine contractions may not be sufficiently powerful to check the blood supply, consequently the nutrition of the organ may continue almost as active as previous to delivery, and accordingly the uterus will remain in a state which may be considered as one of permanent congestion terminating in hypertrophy. Instances of this are very numerous. A similar result may follow in a healthy muscular woman if she leave the recumbent posture too soon after delivery, and, as many of the lower orders do, return to her ordinary occupations, long before the uterus has regained its normal size. Again, pelvic inflammation in any of its varieties is a common cause, interrupting and often arresting the involution of the uterus. Subinvolution may follow on abortion, even when it occurs in the early months of pregnancy, a fact you should not overlook; indeed, my experience leads me to think it is more likely to occur after abortion than after labour at the full term.

The reason why this should be so is easily understood. At the termination of the full term of gestation the uterus is prepared to undergo a process of fatty degeneration and absorption; in fact, this process seems in some cases to commence concurrently with, if not actually prior to, the first symptoms of labour, and it is the natural termination to parturition, the growth of the organ having ceased some time previously, but in cases of abortion not only is the uterus not prepared for the process by which the increase in its size is reduced, but it is called in to undergo this reduction while in a state of rapid growth. The ovum being expelled the growth of the uterus always ceases, but very frequently no attempt is made by Nature to reduce the size of the organ, and this is specially marked when a woman has aborted several times in succession.

The principles which should guide you in the treatment of subinvolution of the uterus are alike in all cases, but the actual method you will follow, and the agents you will employ in carrying out these principles, will necessarily vary in each case. As it is recent, or of long standing, complicated by the existence of congestion, of inflammation, or of hypertrophy of the uterus, its appendages or the neighbouring viscera, or by the presence of granular erosion, retroflexion of the fundus, &c.

Subinvolution of the uterus is of such common occurrence, and is met with under such varied conditions, both as regards duration, complication, and symptoms, that it would be impossible for me to enumerate all the phases it presents, or to mention all the points of interest connected with this condition, you must, by watching the cases under treatment in the wards of this hospital and in the out-patient department, endeavour to make yourself familiar with these, but you will find that all these may be roughly divided into three classes, each class being, in point of fact, a different stage of one and the same affection, it being of course understood that many cases will be met with holding a more or less intermediate position between one or other of these classes.

1st. We have those in which the affection is of recent date, menstruation being profuse, the uterus being soft and engorged, as well as large, the os and cervical canal being patulous, the vagina often being tender and inflamed.

2nd. Cases of longer standing, in which the extreme softness of the uterus no longer exists, though induration has not as yet occurred, there is generally copious uterine catarrh, with more or less erosion of the lips of the os uteri, menstruation being, as a rule, profuse.

3rd. Still more chronic cases, in which hypertrophy of the whole organ, and especially of the fundus, has taken place, with induration of the cervix, the fundus, which is frequently retroflexed, is tender to the touch, and sexual intercourse consequently often painful. Menstruation sometimes continues to be profuse, but very often, as time elapses, becomes scanty.

(1). The cases comprised in the first class—namely those of recent origin, do not come under observation nearly so frequently as do the others. If women do not make a good recovery after parturition they generally think that their sufferings are due to mere debility, and that time will set them right; they may be nursing, though unfit to do so, and menstruation may not have as yet regularly recurred; the pain in the back, the copious leucorrhœa, and the inability to walk may not be deemed of sufficient importance to demand special treatment; but still, a considerable number of women do, from time to time, come under observation within two or three months after their confinements, in whom the uterus will be found to be enlarged, soft, and congested, the cervical canal patulous, and pouring out a copious muco-purulent discharge. The soft and heavy body is very liable to bend backward, and in time to become permanently retroflexed. In such cases the indications for treatment are clear. The distended blood-vessels of the uterus should be relieved by the abstraction of blood, its muscular fibres should be stimulated to contract by applications made to the interior of the uterus, and by the exhibition of ergot, strychnia, &c. The fundus, if retroflected, should be supported and retained in as nearly as normal a position as possible by means of a pessary, and, at the same time, every effort should be made to invigorate the general health. It is in such cases as these that postural treatment, judiciously carried out, is often of so much value. Menstruation, if profuse, must be restrained, even if for that purpose plugging the vagina becomes necessary. Should the woman be nursing she should be made to wean the child.

It is a very hazardous proceeding to apply leeches to the cervix in these recent cases; their application would almost certainly be followed by dangerous hemorrhage, but blood should be extracted from the uterus at short intervals by puncturing the cervix with a sharp lancet-pointed knife, by which means the quantity taken can be regulated to the greatest nicety. When the cervix is very soft and congested puncturing has to be done with care, otherwise profuse bleeding may occur. The point of the knife should not, in the first instance, penetrate to a greater depth than about the 1-8th of an inch, if blood does not flow sufficiently freely another somewhat deeper puncture may be made. The quantity of blood taken at one time should be small—from half an ounce to an ounce is quite sufficient to relieve the local congestion; sometimes even a lesser quantity suffices, but the operation must be repeated at short intervals of two or three days. Should the bleeding be at all profuse it can at any moment be restrained by placing against the os uteri a small pledget of cotton to which a piece of string should be affixed, so that the patient can remove it herself after a few hours. By adopting this precaution you may feel perfectly sure that no undue loss of blood will occur; indeed, if the patient be seen in your own consulting rooms, or in the out-patient department of an hospital, it is generally wise to adopt this precaution before dismissing her.

Various agents are used for the purpose of stimulating the uterus to contract and of lessening the irritability of the intra-uterine mucous membrane, all of these must, to produce any satisfactory effect, be applied to the interior of the uterus, and, indeed, be carried up to the fundus; those most commonly employed are—(1) carbolic acid; (2) tincture of iodine; (3) iodine dissolved in carbolic acid (a preparation to which the name of iodised phenol has been given); (4) nitric acid; and (5) the solid nitrate of silver. I give them in the order in which I recommend their employment.

In recent cases of subinvolution I seldom employ any agent except carbolic acid, it is mild in its effects, seldom causes much pain, and, with care, is quite safe. Let me here give you one rule from which there should be no exception—namely, never apply carbolic acid or any other agent to the cavity of the uterus unless the os internum is sufficiently patulous to permit of the easy passage of the probe through it. If you neglect the observance of this rule you will certainly get into trouble. Carbolic acid, tincture of iodine, or the iodised phenol should be applied about every third or fourth day, and if much congestion be present the cervix can be punctured immediately upon the application being made. This will have the double effect of relieving pain should the carbolic acid have caused any, and by lessening the congestion facilite the contraction of the muscular fibres of the uterus. Carbolic acid applied to the surface of the uterus makes a superficial slough; in fact, it blisters gently the intra-uterine surface. At the moment of application some pain is usually felt, but this rapidly dies away, and, indeed, it often relieves previously existing pain, especially when that is due to hyperæsthesia of the uterus. Nitric acid or the nitrate of silver are not suitable in recent cases.

I have already said that postural treatment is of much value in cases of subinvolution. By postural treatment, I mean the keeping the patient mainly in the recumbent position, and thus preventing the vessels of the uterus and its appendages from becoming unduly distended with blood. But this method can be carried too far. A patient should never be confined to bed, or even to a couch, for such a length of time as to injure her general health. Consequently, I advise you to allow your patients to take a moderate amount of exercise in the open air daily. Walking for a short time is probably the best kind of exercise she can have, but she must not stand about, and the moment the walk is over let her lie down; if she be unable to walk, or that it causes pain, driving in an easy carriage or Bath-chair must be tried, in fact, the patient should lie almost constantly, except when taking exercise. But many patients cannot, or will not, lay up; you must then content yourself with pointing out how injurious standing or sitting upright for a length of time is, warning the patient at the same time that she will retard her recovery by so doing. As already mentioned, the fundus is frequently flexed in these cases; when this is so, a properly adapted pessary not only often relieves pain, but materially aids our other treatment.

(2.) The condition I have described being overlooked, or neglected, the affection passes into another stage. The uterus is no longer unduly soft, but feels firm to the touch, both the body and cervix being enlarged, and the fundus very frequently retroflected, pressure on any part of the organ with the finger causes pain; in fact, hyperæsthesia of the uterus exists, and this explains two symptoms, one or both of which are now commonly present—namely, the distress which walking so commonly causes, and the pain the patient often suffers during sexual intercourse. Menstruation still, in far the majority of cases, continues to be unduly profuse, while, on looking through the speculum, more or less extensive erosion of the lips of the os uteri will generally be observed. Copious uterine catarrh is also nearly always present.

Here, as in the former case, you have to direct your attention to the lessening of congestion and the reduction in size of the enlarged uterus, and to the relief of pain; but your task is now much more difficult, you will often find it hard to obtain sufficient blood by puncturing the cervix, though this generally can be effected by making the knife penetrate to a greater depth. the hot water vaginal douche sometimes acts exceedingly well in these cases, and its use should never be omitted. The application of carbolic acid, or better, the iodised phenol, is still needed, and in some obstinate cases nitric acid should be employed. But none of these agents are to be used till the tenderness of the uterus is lessened, and any symptoms of inflammation which exist removed; in fact, you are now dealing with a state of chronic inflammation, the result of neglected subinvolution of the uterus. I must, however, defer till a future occasion entering into details of the treatment you should adopt under these altered circumstances.

(3.) The third class comprises those cases which have passed into a still more advanced stage. The walls of the uterus are thickened and hypertrophied, though the actual depth of the uterine cavity, as proved by the use of the sound, may be less than previously. The fundus and cervix are still generally tender to the touch; and if the body has been flexed in the early stage, the flexion is now incurable. Menstruation in the majority of cases, instead of being profuse, now appears in diminished quantities, though in a few the flow is still excessive. The patient nearly always complains that walking causes distress, and of a sense of fulness in the pelvis, and a distressing feeling as though the uterus were prolapsed. In fact, you have hypertrophy, with hyperæsthesia of the whole uterus, associated with which there is an unhealthy condition of the intra-uterine mucous membrane. The heavy uterus sinks low in the pelvis, and presses on the viscera, and its natural supports being now wanted, it receives a jar from every motion. Here you have to deal with one of the most troublesome and difficult forms of uterine diseases which it is possible to conceive, and one which will tax all the resources at your command to the utmost.

Local blood-letting now is of comparatively little value, but it may be tried. The action of carbolic acid is too feeble to produce much effect on the hardened uterus. Iodised phenol will do better; but even that agent, strong though it be, is generally insufficient. So, too, is even nitric acid. On the whole, I think in these cases that the solid nitrate of silver introduced into the cavity of the uterus is the most efficient agent. Still I employ it very seldom, and only have recourse to it in those cases in which, menstruation being profuse, the other agents I have mentioned have prove inefficient. The free use of the thermo-cautery sometimes is of great use in these chronic cases. Pessaries in general, seldom suit them. Often they cannot be borne. The one I have found most beneficial in relieving the distressing bearing-down sensation, is Salt's watch spring pessary. The chief objection to it is, that being covered with soft rubber, it is acted on by the vaginal secretions, and rapidly becomes coated with a deposit, and consequently must be removed at short intervals for the purpose of being cleansed.

The sufferings endured by patients whose cases have passed into the third stage I have just described are often great and, unfortunately, lasting. I have known young women under thirty years of age to become permanent invalids, with little hope before them of obtaining relief from their sufferings. Time sometimes does much; and if the patient can rest she may gradually improve, but among those who have to exert themselves this seldom occurs.

In thus sketching the outline of the stages through which the uterus passes when its involution has been defectively performed, you must not suppose that there is any well-defined division between them. One passes imperceptibly into the other, and numerous modifications of the symptoms and conditions I have described are daily to be met with, as well as many complications from irritation or inflammation of the neighbouring viscera, especially of the bladder, while the digestive functions nearly always become imperfect. Nor are you to suppose that all this long train of symptoms sets in always at once, or dates necessarily from the most recent pregnancy. On the contrary, it often commences long previously; perhaps after the patient's first or second labour, involution of the uterus was imperfectly performed; still the organ remained sufficiently healthy to permit of pregnancy again occurring, and of utero-gestation proceeding to the end of the full term, then labour occurring, unless great care be taken—and sometimes even with the greatest care—involution is still more imperfect than before, and so the case proceeds, till the uterus, unfitted for the office it should perform, allows of the escape of the immature ovum, and a bad miscarriage or premature labour occurring, all the patient's sufferings are laid down to it, whereas the evil had commenced a long time previously.

In dealing with weakly, delicate women, it is sometimes impossible to prevent the occurrence of defective involution after parturition, but these cases are the exceptions; and without doubt all the troubles and sufferings I have described are in general preventible, and such of you as are engaged in the practice of midwifery, should inculcate on your patients the absolute necessity which exists for their giving the uterus time to recover itself after childbirth, and you should point out to them the risks they run if they neglect your advice. This is clearly your duty, and it is a duty which should never be neglected.—*Medical Press and Circular, Jan.* 18, 1882, *p.* 41.

81.—NOVEL TREATMENT OF INVETERATE AND TROUBLESOME DISPLACEMENTS OF THE UTERUS.

By William Alexander, M.D., F.R.C.S., Liverpool.

The cure of uterine displacements by operative methods is never entertained in cases where the malposition is recent and can be rectified by reposition and rest, or by instrumental appliances that do not distress the patient. There are, besides the cases included in the above-mentioned category, a great number of others whose lives are a torment to them by reason of the inefficiency or irritation of pessaries. The fact of every gynæcologist of any note using a pessary or pessaries of his own invention proves that all but the most recent and untried ones are often ineffectual. Many women, again, have a natural dislike to wearing such instruments, and to the periodical re-adjustments which their use necessitates; and, although the misfortune of displacement is in them permanently palliated by the pessary, they would willingly undergo any reasonable safe operative procedure that would be likely to release them from manipulations so distasteful to them. Such an operation is that which I now intend to describe.

Before I thought of it, I performed the only operation hitherto described for prolapse of the uterus, in which the vaginal canal is contracted and the perineum fortified by means of plastic operations that are as numerous as the operators themselves. Although these operations do good in a certain number of cases, their success depends on the absence of all dilating causes. The external dilating causes *can* be abstained from, but *are* not; whilst the internal causes are always at work, and hence in the very worst cases, where operation is most necessary, the result is the least satisfactory. I have performed several operations by the old method, and only in one was I quite successful, after the lapse of three months. In that case I had to modify the operative methods of preceding operators in a radical and useful way; but as since that time I have devised a better method, applicable to all cases, I will not refer to the case further.

My method of operation was led up to by several considerations of an anatomical character, which I will first refer to. In the performance of gynæcological operations on the cervix uteri it is a well-known fact that even the virgin uterus can be drawn down with forceps until the os presents just inside the labia. The multiparous uterus can be extruded further and with much more ease. The resistance to further extrusion arises from the peritoneum connecting the uterus to other organs, and from the broad and round ligaments. The round ligaments do not resist the extrusion of the uterus so much as the other structures. Their special function I will in a moment mention.

The force required to pull down the uterus is very small, and far inferior to the pressure of the abdominal contents upon the pelvic outlet during straining. We know how peritoneum can stretch or yield in hernial protrusion, so that if the perineum and the support of the vaginal walls were the only obstacles to prolapse of the uterus, prolapse would be the rule, and not the exception. In other words, if the uterus were an intrapelvic wedge, so placed as to be supported only by the vagina and perineum, and its axis so directed that the intrapelvic pressure could act upon its fundus in the direction of the vaginal canal with the greatest available force, no woman could be found with her uterus unprolapsed.

The uterus, when in the normal position, is scarcely subjected to any force that tends to produce protrusion. The sacrum and coccyx bear the greater part of the pressure in one direction, and the pubic bones bear the pressure in another direction. Whatever maintains the uterus in this normal position is the great preventive of prolapse. For the virgin uterus, the natural resilience and coaptation of the surrounding organs is probably sufficient; but when these fail, the *round ligaments* are the chief agents. These ligaments are "attached to the upper angles of the uterus, one on either side, immediately in front of the Fallopian tube. From this point each ligament proceeds upwards, outwards, and forwards, to gain the internal inguinal ring; and after having passed, like the spermatic cord in the male, through the inguinal canal, reaches the fore part of the pubic symphysis, where its fibres expand and become united with the substance of the mons veneris. Besides areolar tissue and vessels, the round ligaments contain plain muscular fibres, like those of the uterus, from which, indeed, they are prolonged." (Quain.)

These ligaments, so placed as to manifestly prevent backward or forward swaying of the fundus uteri, are unfortunately subject to stretching, as nearly all soft tissues are, provided the force be persistent enough. This force arises in repeated pregnancies, uterine congestions, and from other causes well known to gynæcologists, that change the position of the uterine wedge, and overcome the *staying* force of the round ligaments by stretching them. If complete retroflexion or retroversion takes place, prolapse is not so serious; but it is when the retroversion is partial, when the uterus comes more into a line with the axis of the pelvic outlet, that the pressure of the abdominal contents acts with most efficiency, and with a persistence that is certain to produce protrusion, if not complete prolapse.

If we could replace the uterus, and maintain it when replaced in its normal position and direction, we should at once take away its tendency to prolapse. We cannot easily increase the resiliency of the surrounding tissues, but we can do what my operation consists in, and that is "*pull out the slack of the round ligaments.*"

The operation is performed by cutting down upon each abdominal ring, gathering up the ends of the ligaments, freeing each from its nerve, and gradually releasing them by patient and cautious traction from the neighbouring tissues, until the position of the uterus, as ascertained by the finger in the vagina, satisfies the operator. The ligament is then stitched to the tissues around the ring, and the loose ends attached to each other or rolled round two pieces of wood, which are fastened together in the middle line. The picking up of the ends of the ligament is the *difficult* point, and the freeing of the ligaments from their surroundings is the *delicate* point, but by experience both can be performed easily and effectually. The ligament slides within its sheath, and the peritoneum is not disturbed. No risk of hernia or of pelvic inflammation occurs. Beyond some pain for the first few days, the operation is harmless if carefully performed, but experiments on the dead subject have shown me that danger may arise from incautious operators.

Case 1.—Elizabeth C., aged thirty-eight, a well-nourished woman of medium stature and relaxed pelvic organs, was admitted into the Liverpool Workhouse Hospital, suffering from prolapse of the uterus to such an extent that the cervix presented outside the labia. On Oct. 4, 1882, I performed the vaginal operation, and narrowed the vagina to a finger's width. On Dec. 1 the uterus did not appear externally, but was pressing forcibly downwards upon the narrowed vagina. The bladder and rectum were bulging forwards and backwards as if all these organs were endeavouring to escape by the vaginal outlet. The dragging pains that the woman suffered from were unabated, and the only improvement was that we did not see the uterus with the naked eye when the vulva was exposed. On Dec. 14 I performed my operation on the round ligaments. Two inches of the slack of each were pulled out and cut off, and the ends stitched by numerous catgut sutures to the boundaries of the wounds. On Jan. 6 the wounds were quite healed, and a dimple in the centre of the cicatrix showed where the ligaments had formed their attachments.

The patient complained of some pain during the first two days. This was relieved by morphia injections. I examined the patient on Feb. 27 in the erect position. The uterus remains fixed in the position into which it was drawn at the time of operation. Coughing and straining does not tend in the least to produce prolapse. The cervix merely sinks a little downwards and forwards in the direction of a normal uterus, but not quite so much as a normal multiparous uterus would sink. The reason of this is that the ligaments have been made more "taut" than the normal ligaments ever are; and this increased support is necessary where these ligaments only have to be depended upon. The vaginal walls are still relaxed, but not so much as at the time of operation, and it is to be expected that, when the dilating wedge is removed, the stretched tissues will recover somewhat of their former resiliency if the stretching process has not already gone too far. The dragging pains have left her completely, and the woman is quite at liberty to rejoin her husband. Pregnancy is not an event likely to occur in the present case, or in many of the cases upon which operation for prolapse is necessary. But if it should occur, one of three things might happen — the curtailed ligaments might refuse to allow the uterus to rise into the abdomen, and abortion would result; or the ligaments might stretch sufficiently to allow the uterus to rise, and might recover their tone after the birth of the child; or they might fail to recover their tone after delivery, and the prolapse would recur. When cohabitation has been provided for, the chief object has been attained, and the superiority of the present operation over all others vindicated. Pregnancy completely destroys the results of the lower operation, and, theoretically speaking, it may not destroy the results of this the higher operation.

Case 2.—Bridget K., aged forty-five, was admitted to hospital on Jan. 15, 1882, with chronic prolapse of uterus and bladder, from which she had suffered for many years. She was operated on on Feb. 1, and two inches of the slack of the round ligament pulled in. This patient had an attack of bronchitis, induced by ether, upon the chronic form of the disease, from which she has suffered for some time. The constant coughing increased the pain of the wounds, and her temperature ran up to 101° during the first three days. The wound did not heal by the first intention, but granulated. On Feb. 20 the wounds were healed and the uterus fixed in the position in which it was placed at the time of operation. On Feb. 27 the results are the same as in the preceding case. The pressure of straining does not affect the uterus, and the maintenance of the cure is certain. Bronchitis is a barrier to the performance of the operation, from the pain produced by the coughing, and where there is any tendency to it I would recommend the use of chloroform in preference to the use of ether.

Case 3.—Ellen T., aged twenty-eight, has had a prolapse of the uterus and anterior vaginal wall for several years. She has been in and out of hospital for the last year and a half, and treated without effect by various kinds of pessaries. I had contracted the anterior vaginal wall without effect, and was about to operate on the posterior vaginal wall and perineum, when the idea of the round ligament presented itself to me. On Jan. 4, 1882, I shortened her ligaments. Her temperature never rose above 100°, and she only complained of some pain for three days. I examined her in the erect position on Feb. 27, and found the uterus in position, and affected by coughing and straining in the same way as the other two cases. This patient is an epileptic, and is at present in hospital for the treatment of that disease. Although I first practised this operation for prolapsus uteri, the very principle upon which it is founded, and the way in which it acts in prolapse, imply that it is a radical cure for retroversions and retroflexions of the uterus.

Case 4.—Elizabeth D., aged thirty years, was admitted on Sept. 17, 1881, with retroflexion of the uterus of a most marked kind. Pessaries failed to relieved her, and the menstrual period was one of great trouble to her, and was marked by the occurrence of epileptic fits. The operation was performed by pulling out the round ligaments as before. In this case I failed to catch up one ligament properly, and by only one was the uterus brought into position. There was scarcely any rise of temperature, and the wound had healed soundly by Jan. 13. I examined this woman on Jan. 30. The retroflexion is completely cured, and the sound passes in the normal direction. The epileptic seizures were unaffected by the operation, although the distress experienced at the menstrual periods is completely cured. She is now undergoing some medicinal treatment for epilepsy preparatory to operative treatment.

The only other case of retroflexion in which I have performed the operation was on a patient of Dr. Irvine's. The wounds have not yet healed, and I merely mention the case now to say that about a week after the operation she menstruated, without any pain. This she had not done for years. In extreme cases of anteversion I believe the operation would be as useful as in retroflexion or as in prolapse, but I have not had an opportunity yet of proving its efficacy.

It seems to me rather surprising that such a simple, rational, and effectual operation had not been thought of before. The operation is entirely extra-abdominal, and lethal consequences can only follow where erysipelas, pyæmia, or other causes that may produce death in the simplest wound, come into play. Before anyone performs it, I would recommend considerable practice on the dead subject, by which the "knack" of seizing and pulling out the ligament can be discovered much more easily than by reading any lengthened description that I could possibly give.—*Medical Times and Gazette, April* 1, 1882, *p.* 327.

82.—DILATATION OF UTERUS BY HEGAR'S DILATORS.

By Dr. **Macan** and Dr. **Atthill**, Dublin.

Dr. **Macan** exhibited lately at a meeting of the Dublin Obstetrical Society, a set of Hegar's Dilators and said:—Of late years, on account of the danger of septic infection attending the use of the ordinary methods of dilating the uterus, whether by sponge, laminaria, or tupelo tents, numerous attempts have been made to dilate the uterus quickly by means of instruments specially invented for this purpose. One class of these instruments have two or more branches, which are introduced closed into the cervix and then forcibly separated. To this class belong the dilators of Priestley, Ellinger, and Busch. The chief objections to such instruments are that they dilate the cervix unequally, and their complicated nature renders their thorough disinfection almost impossible. The other class of dilators is represented by the one I now lay before you. The idea of dilating the cervix by graduated bougies is not original with Hegar, but was first proposed by Peaslee, and both Hanks and Lawson Tait have introduced graduated uterine dilators made of vulcanite. Hegar's dilators differ, however, very essentially from these, both as to their shape and also as to the number of dilators that are considered requisite. Hanks' dilators consist of a set of ten hollow vulcanite ovoids, which can be screwed into a common handle. Mr. Tait's dilators are only four in number, are conical in shape, and also fit into a common handle. Hegar's consist of twenty-five pieces, each piece being about 5 inches long and cylindrical in shape. At one end the cylinder is flattened out for a distance of 1¼ inches, so as to form a sort of handle. The first piece has a diameter of 2 millimetres, and the diameter of each successive piece increases by half this amount, or 1 mm.

In order to introduce them the woman is placed on her side in Sims' position and his speculum is introduced. The cervix is then fixed with a vulsellum, and then one of the dilators is introduced till it passes the internal os. If it goes in quite readily, it is at once removed and the next larger size introduced. If, on the contrary, there was much difficulty experienced, the dilator is left for from 5 to 10 minutes *in situ*, at the end of which time the next number can be easily introduced. As a rule, we may commence with No. 5-7 in women who have borne children, and with No. 3-4 in those who have not. Each dilator is stamped with the same number as there are millimetres in its diameter. It is a curious fact that the resistance to dilatation sometimes becomes much less after a certain amount of dilatation has been effected.

If our object be merely to apply intra-uterine injections with Braun's syringe, or to use a small curette, we need not go further than No. 6 or 7. If we want to apply nitric acid to the cavity, it is better to go on to No. 10 in order to be able to introduce a small speculum to protect the cervix. If we wish to introduce the finger, it will be necessary to go on to No. 15-17, and if a fibrous tumour is to be removed we must go as high as No. 26, or even higher.

Under no conditions are the advantages of these dilators better seen than in cases of imperfect abortion. Here we can readily introduce them up to No. 17, and then remove anything that had been left behind. They may also be used to induce abortion or premature labour, and have proved most useful in cases of placenta prævia, for which, however, higher numbers are requisite.

As a rule, it is possible in an hour to produce sufficient dilatation of the unimpregnated uterus to admit the introduction of the finger as far as the fundus. The only contra-indications to their use is any inflammatory condition in or around the uterus.

Before being used the dilators are steeped for several hours in a five per cent. solution of carbolic acid, and are dipped in five per cent. carbolic oil to facilitate their introduction.

I have used these dilators myself several times, and am satisfied that they are superior to all other methods that have been proposed for the quick dilatation of the cervix. Dr. Atthill has also kindly given them a trial on my recommendation, and will, I am sure, give the Society the result of his experience of their use.

The whole process is greatly quickened by having the difference between the diameters of successive dilators reduced to half a millimetre instead of 1 mm., and by putting the patient under chloroform. It has occurred to me that the dilatation might be more easily and quickly effected if each dilator were slightly cylindrical, the diameter at the point being 1 mm. less than at the handle. Then the point of each dilator would be of the same diameter as the opposite end of the dilator that had preceded it. I have therefore ordered a set of dilators thus modified.

DR. ATTHILL, said I have satisfied myself that Hegar's bougies are superior to any other form of uterine dilators. I have used them four times, but have not yet tested them thoroughly. As a general rule, I am opposed to rapid dilatation of the uterus. I believe slow dilatation by means of sea-tangle to be the safest method we can employ. The pain is not great, provided the case be a proper one for dilatation. If you dilate in an unsuitable case, as where the cervix is conical, it is otherwise, but in such cases dilatation should not be attempted at all; and I have never seen septicæmia ensue after their use, when dilatation was carefully conducted, and care taken to wash out the uterus with some disinfecting fluid on the removal of the sea-tangle. If you dilate with sponge-tent, the risk of septicæmia is greatly increased, because you cannot use sponge-tent without lacerating the mucous membrane of the cervical canal. The mucous membrane sinks into the cells of the sponge as it swells, and must be torn during its removal. I hold that the dilators now before us are not suitable when the cervix is rigid, nor yet is conical, and the os uteri small. In such any attempt at rapid dilatation is exceedingly dangerous, and I never would venture upon it, even with Hegar's bougies. But I believe rapid dilatation by means of these instruments is suitable in many cases in which you have a soft, relaxed cervix. Take the case where a small portion of the membranes of the ovum or of the placenta is retained for some weeks after abortion. Here you have a soft cervix, capable of being dilated rapidly without any risk. Such a case came under my care recently. I commenced with the introduction of No. 5 bougie, and as the cervix was soft, in less than twenty minutes No. 14 was reached, and then I was enabled to pass my finger into the uterus, and detach and remove a portion of the placenta not larger than a bean. I gave chloroform in that case. Next I attempted to use these in the case of a woman who had not been pregnant for many years. This woman was suffering from profuse hemorrhage. After I had got in No. 5 I found the resistance of the cervix so great that I ceased altogether. In that case also I gave chloroform. I am now of opinion that that woman has malignant disease of the fundus, though the cervix is healthy. Subsequently I effected dilatation satisfactorily by means of sea-tangle, and removed portions of a vascular growth, but the pain and hemorrhage have returned, and the woman's condition is very bad at present. I have a lady under my care in whom I dilated the uterus three times with sea-tangle. On each I removed a vascular growth, but all our efforts have failed to cure her. I am obliged every week to inject astringent solutions to keep the hemorrhage in check. I believe there is a malignant growth filling up the uterus, which renders the insertion of even the tube of a small syringe difficult. Since I obtained Hegar's dilators they have facilitated my treatment considerably. I introduced three or four in succession, on each occasion without giving the patient any pain. Then I am able to introduce the tube of the syringe, and inject some iodine without trouble. I have no hesitation in saying that Hegar's are by far the best dilators I have seen. I have tried Lawson Tait's several times, and have found them most unsatisfactory. They are too short and conical.—*Dublin Journal of Medical Science, March*, 1882, *p.* 230.

83.—ON THE RELATION OF ANTEFLEXION OF THE UTERUS TO DYSMENORRHŒA.

By G. Ernest Herman, M.B., M.R.C.P., F.R.C.S., Examiner in Midwifery to the Royal College of Surgeons, &c.

In patients who suffer from painful menstruation, anteflexion of the uterus is often found. This fact has been familiar to the profession for half a century, and the interpretation has been put upon it, that the anteflexion causes the dysmenorrhœa. This interpretation has been accepted and acted upon by many gynæcologists of the greatest reputation, and denied by others of equal eminence. An inquiry into the nature of the connection between painful menstruation and bending forwards of the uterus is therefore not uncalled for.

The mode in which flexion of the uterus is commonly supposed to cause dysmenorrhœa is the following: — It is said that at the point of flexion the uterine canal is bent at an angle, and its wall on the concave side of the bend projects inwards, forming a spur or promontory, which blocks up the passage and prevents the menstrual fluid from getting out. The retained secretion distends the uterine cavity, and provokes painful contractions of the uterine muscular fibre. Sometimes the uterus cannot force the pent-up blood past the barrier formed by the flexion, and it may then be driven along the Fallopian tubes, forming a pelvic hæmatocele.

I would here make the assertion, which can be refuted by the production of a single case, that *there is no anatomical evidence that anteflexion of the uterus causes any hindrance to the escape of the menstrual fluid.*

It may be urged, however, with reason, that this absence of anatomical proof does not show the theory to be unsound, because evidence of this kind is difficult to get. Dysmenorrhœa is seldom, if ever, fatal; if fatal at all, it would only be after so many secondary changes, inflammatory and other, had taken place, that it would be impossible to isolate those directly and originally dependent on the flexion. Diseases of other parts of the body which cause death, very commonly themselves produce changes in the genital organs and alterations in the menstrual functions, so that inferences cannot safely be drawn from them. When death results from accident, it is seldom that we have the opportunity of previously making full inquiry into the condition of menstruation. The nature of an obstetrician's duties prevents him, as a rule, from frequently making post-mortem examinations; and therefore it generally happens that in large hospitals these are made by a member of the staff who has no particular interest in, or knowledge of, gynæcology. These are all reasons why we might expect anatomical proof to be furnished but slowly.

On the other hand, it is now more than fifty years since the doctrine of the importance of uterine flexions was promulgated; during that time numbers of gynæcologists have directed their attention to the subject, and the medical press of all countries has teemed with papers upon it; it is therefore very surprising, seeing how common a condition anteflexion is, that if it really produce such changes, no one should during all that time have met with, or thought it worth while to put on record, a case illustrating them.

The clinical fact which has procured this theory such wide acceptance, is the undeniably large number of cases of dysmenorrhœa in which the uterus is found anteflexed. Many gynæcologists have published statistics, compiled from their notebooks, showing in how large a proportion of cases they met with anteflexion, and how often it was associated with the different symptoms ascribed to it. But before we can conclude from the frequent co-existence of the phenomena in question, that *because* the uterus is bent forward *therefore* the patients menstruate painfully, we must know how often the bending occurs without any alteration in the functions of the organ. The practice of an obstetric physician can never give this information, because the patients whom he examines come to him on account of some disorder of the genital organs, very often dysmenorrhœa; he does not have an opportunity of examining women who think their uterine functions are naturally performed, and therefore he cannot tell in how many of them the uterus is flexed.

Out of 111 patients, pronounced anteflexion was present in 53, or 47·7 per cent. The proportion of anteflexions which my investigation revealed agrees so closely with the figures I have quoted from other authors, that I think all of them together justify the assertion that *in nearly half of all nulliparous women the uterus is anteflexed.*

The fact, however, that anteflexion is exceedingly common is no proof that it does not cause menstrual pain. It may be asserted that although the slighter forms are insignificant, yet that the more acute flexions cause suffering. It may also be said that a flexed uterus which may give no trouble so long as its owner remains single, may yet, when subjected to the altered functional conditions involved in marriage, give rise to painful symptoms. It is found that out of 57 women in whom the uterus was only slightly or not at all flexed, in 40, or 70 per cent., there was little or no pain at the menstrual period; and in 17, or 29 per cent., there was severe pain. Out of 53 women in whom there was pronounced anteflexion, in 37, or 69 per cent., there was little or no pain at the menstrual period; and in 16, or 30 per cent., there was severe pain. Classifying them differently, so as to show what influence the *acuteness* of the flexion has, it is found that out of 23 cases with acute anteflexion, in 16, or 69·5 per cent., there was little or no menstrual pain; in 7, or 30·4 per cent., severe pain. Out of 87 others, in whom such anteflexion as was present was not acute, in 61, or 70·1 per cent., there was little or no menstrual pain; in 26, or 29·8 per cent., severe pain.

The differences between these two sets of figures are so trifling that I think them practically *nil*. The figures show two broad facts:—1. That menstruation is as often painful when the uterus is straight as when it is anteflexed. 2. That it is as common for menstruation to be painless when the uterus is anteflexed as when it is not anteflexed.

With regard to the view that married life might cause the development of symptoms in an anteflexed uterus, I would say, that most of the subjects of my investigation were prostitutes; and one would think that whatever injurious effect upon the sexual organs ordinary marital intercourse might have, would also result, but much intensified, from the mode of life of a prostitute.

It seems to me that facts point clearly to the inference *that the degree of bending forward of the uterus does not appreciably influence the presence or severity of menstrual pain.*

If anteflexion generally causes dysmenorrhœa, it ought to be the case (1) that, as a rule, all treatment of such dysmenorrhœa which does not straighten the uterus should be inefficient, and (2) that treatment which does straighten the uterus should as a rule be successful. I insert the words "as a rule" because clinically cases are often complicated, and general assertions true of simple cases, may not hold good when applied to a combination of morbid conditions.

1. The distinguished physician to whose able advocacy and high reputation the wide acceptance of the mechanical theory of uterine pathology is mainly due, has advocated rest in the recumbent posture as the "first principle of treatment" in anteflexion. However useful this treatment may be, and whatever its efficiency in straightening the uterus, there are so many morbid processes which tend to recovery when the

patient is kept at rest, that the disappearance of symptoms under this regimen can hardly be held to be conclusive as to their origin.

2. Dr Graily Hewitt has also invented for the treatment of anteflexion a most useful pessary, the "cradle." I have not seen any statement of the frequency with which benefit to dysmenorrhœa has followed its use. I have known menstrual pain diminish and cease while the patient was wearing one of these pessaries. But I have never found the uterus straightened while it was being worn. I do not think the cases are many in which it will remove dysmenorrhœa; but when it does so, it is without straightening the uterus.

3. It is well established that there are cases in which enlargement of the cervical canal by incision cures dysmenorrhœa. Acting on the theory that dysmenorrhœa occurring with anteflexion is due to narrowing of the canal owing to the bending, Dr. Marion Sims has planned an operation in which the incisions are so directed that, if extensive enough, they would make the canal straight. I have not seen any published cases showing the result of that operation, and what proportion of cases it relieves; but it is quoted with approbation by several distinguished compatriots of the author; and it is scarcely conceivable either that they would adopt, or that a man so eminent as Dr. Sims would promulgate, an operation which had never been followed by at least apparent benefit. But any one who will examine a section of an acutely anteflexed uterus will see that it is impossible by incision to obliterate the bend of the canal, except by cutting so deeply as to reach either bladder, or peritoneum, or both. This anatomical argument applies only to the fresh incisions; after they have healed, the space gained by them would be much less than when recently made. An operation in which the incisions were no deeper than they could be safely made, if it widened the canal, would not make it straight; and cicatrization would restore it nearly, if not quite, to its former condition. Therefore, Dr. Sims's operation, if successful, is an instance of the cure of dysmenorrhœa with anteflexion without straightening the uterus.

4. It is well known that there are cases of dysmenorrhœa in which benefit follows dilatation of the uterine canal, either by bougies, tents, or an expanding sound. This has been claimed as evidence that the anteflexion is the cause of the dysmenorrhœa, it being assumed that the effect of the dilatation is simply mechanical, that there is stenosis, and that the anteflexion is the cause of the stenosis. It is impossible here to discuss this very wide subject. I will therefore confine myself to stating the objections to this theory, which are:—(1) That the therapeutic effect of dilatation is not proportionate to the

amount of resistance encountered by the dilating agent. (2) That dysmenorrhœa, removable by dilatation, is met with when the uterus is straight as well as when it is bent, and (3) that the anteflexed uterus remains nearly or quite as much bent after the dilatation as before it.

5. There is one agent which can, beyond question, straighten an anteflexed uterus, viz. the intra-uterine stem. There is no doubt that in some cases menstrual pain is relieved by the use of these instruments, but their effect is not confined to the straightening of the uterus. It is well known that when an instrument just filling the cervical canal is left there the canal soon dilates, so that the instrument fits loosely. The intra-uterine stem is therefore a dilator of the cervical canal as well as a straightener of the uterus. Besides this, it irritates the organ, causes a determination of blood to it, and usually more or less endometritis. So common an effect is this, that one advocate of the intra-uterine stem has said that he looks upon the occurrence of hemorrhage and free discharge as a favourable sign. It is possible that this stimulant or irritant effect (whichever term be preferred) may in some cases do good. There is yet another source of fallacy. Those who advise the use of stem pessaries lay much stress on properly preparing the uterus, so that it may tolerate the instrument; this preparation consisting in the removal of all inflammation or congestion of the pelvic organs. Seeing how frequent anteflexion is, it is to be expected that, unless it were a prophylactic against these conditions, it would often be associated with them. It may possibly be sometimes the case that benefit seeming to follow the wearing of an intra-uterine stem is really due, not to the instrument, but to the careful preparatory treatment. Benefit following the wearing of the intra-uterine stem may have been due to any one of these three causes, or to the combination of them, and not have resulted from the straightening of the uterus. But if it be true that anteflexion generally causes menstruation to be painful, then the stem pessary, when well tolerated by the uterus, ought to remove the pain as constantly as it without doubt removes the flexion; and in cases of dysmenorrhœa with anteflexion, in which the removal of congestion, vaginal pessaries, dilatation, incision (methods of treatment which do not remove anteflexion), have been tried in vain, the intra-uterine stem ought to be almost invariably successful.

Unfortunately, the literature of the subject does not furnish any scientific statement of the frequency with which intra-uterine stems cure dysmenorrhœa associated with anteflexion. Nearly all authors who have written upon the subject have given only their general impression as to what is the usual result; they have not given their experience in full. Some gynæcologists of ability have thought that they saw much benefit from intra-uterine stems; others, of equal eminence, have condemned the instrument altogether. These general statements seem to me likely to be erroneous, for this reason, that an individual observer tends to see an undue proportion of the cases in which his own treatment has been successful and that of others unsuccessful; for those patients to whom he has done good continue to consult him, while those for whom his treatment has been unsuccessful lose confidence in him, and go to some one else. The only way to avoid this source of error is to carefully record *every case*, so that not only the number of cases cured may be known, but the number of those in whom treatment of the same kind was unsuccessful, was impracticable, or the patient was lost sight of. I cannot find any author who has done this in a way that bears materially on the present question; the cases published in detail are too few to be of use, and those writers who have given their experience in figures do not isolate the cases in which the stem was used for dysmenorrhœa with anteflexion from those in which it was resorted to for different conditions and symptoms. I am obliged, therefore, to say that we have no information as to the frequency with which straightening the uterus with an intra-uterine stem cures dysmenorrhœa associated with anteflexion. I do not think it worth while to give my own experience, because, knowing that these instruments are attended with some risk, and doubting their utility, I have only employed them in a few cases in which many other kinds of treatment had failed; and it might be said that it is not fair to test any treatment by cases selected for their incurability.

I cannot but regard the extreme differences of opinion that still exist as to an instrument which has now been before the profession for more than fifty years, as in themselves very important. The dangers which are believed to accompany it seem to me no adequate explanation of the disfavour with which many regard it. If it were as uniformly successful as by the theory of its action it ought to be, its efficiency as a means of a cure would long since have been acknowledged. There might still be differences of opinion as to its safety, or as to whether the malady was enough to make it worth the patient's while to incur the risk of possible ill consequences, but there would be no dispute as to the efficacy of the stem in removing menstrual pain.

The general conclusions which I draw from the effect of treatment are these:

(1) That dysmenorrhœa associated with anteflexion is frequently cured without straightening the uterus.

(2) That straightening the uterus does not invariably cure the dysmenorrhœa, and that there is no evidence that it does so frequently.

In summing up the purport of this paper, I would submit the following propositions :

1. That there is no anatomical evidence that anteflexion causes any appreciable hindrance to the escape of menstrual fluid.
2. That there is reason to think that well-marked anteflexion is present in nearly half of all women who have not borne children.
3. That therefore it is to be expected that anteflexion and dysmenorrhœa would frequently coincide.
4. That dysmenorrhœa is practically as common when the uterus is straight as when it is anteflexed.
5. That painless menstruation is practically as common when the uterus is anteflexed as when it is not.
6. That when dysmenorrhœa and flexion go together, the severity of the pain bears no relation to the degree of the bending.
7. That dysmenorrhœa associated with anteflexion is frequently cured without straightening the uterus.
8. That there is no evidence that straightening the uterus invariably, or even frequently, removes dysmenorrhœa which is associated with anteflexion, and in which other methods of cure have been ineffectual.
9. That these facts tend to show that the relation between anteflexion and painful menstruation is not that of cause and effect, but merely that of coincidence.—*Obstetrical Transactions*, 1881, *p.* 209.

84.—TREATMENT OF SPASMODIC DYSMENORRHŒA AND STERILITY BY DILATATION OF CERVICAL CANAL.

By Clement Godson, M.D., M.R.C.P., Consulting Physician to the City of London Lying-in Hospital, &c.

It will be well, first of all, to avoid confusion, to describe that form of dysmenorrhœa denoted by the term *spasmodic.* It is that which is characterised by acute pain of a colicky character attacking the womb, referred to the hypogastric and sacral regions, sometimes running down the thighs, coming on either before the menstrual flow makes its appearance, or coincident with it, which may continue for one or two days, but generally subsides after a few hours, leaving behind it a state of prostration and uneasiness lasting probably for several days—a pain which obliges the patient to lie down, often to roll on the floor or bed, which frequently gives rise to reflex symptoms, such as retching or vomiting, and generally causes the patient to moan or call out in her agony, according to its severity. This disease may appear with the first menstruation or later, and then generally occurs regularly with each successive period, more or less severely, is almost always aggravated by marriage, and gives rise to barrenness. It has been called *spasmodic* or *obstructive* dysmenorrhœa. I prefer to adopt the title spasmodic, for it is certain that it is this, and to drop the term obstructive, for I am by no means sure that it is such; the pain is often as severe when the flow is free as before the discharge appears, and I know no evidence to prove that there is a want of patency of the cervical canal, indeed, I have been told by our President that he has, during the very height of the pain, passed a probe into the uterus without meeting with any obstruction, and he has shown, in an important paper in the "Journal of Anatomy and Physiology" of November 1870, written by himself and Dr. Arthur Gamgee, entitled "Notes of Some Experiments on the Rate of Flow of Blood, and some other Liquids through Tubes of Narrow Diameter," how little influence a uterine flexion or other contraction without complete closure can have on the escape of the menstrual discharge. In one experiment a tube was bent four times at right angles in one plane, and at one point at an angle of about 135° to its former plane; even this had no perceptible influence in modifying the flow—the quantity of fluid flowing in the same time being directly as the pressure, and very much influenced by rises of temperature. What is it, then, that gives rise to the intense pain experienced in this form of dysmenorrhœa? Capuron, in his "Traité des maladies des Femmes," Paris, 1817, speaks of it as produced by "une constriction spasmodique du col et de l'orifice uterin;" this, like the term obstructive, seems to imply retention of menstrual fluid by constriction of the uterine orifice. I would suggest that the spasm of the uterine muscular tissue is of itself sufficient to give rise to the severe pain, independent of any obstructing stricture or distension of the cavity of the womb by retained blood, and that this is proved by the fact that the passage of a sound or bougie into the uterus of a woman who suffers from this disease, at any intermenstrual stage will give rise to precisely the same pain and reflex symptoms, simply by the irritation caused by the contact of the instrument, the whole of these symptoms disappearing in time, as soon as the endometrium becomes accustomed to the foreign body; furthermore, as will later on be shown by a case quoted, that the constant contact of a foreign body seems to blunt the sensibility of the hitherto irritable structure.

It seems tolerably certain that the most sensitive part is the so-called internal os, that portion of the uterine cavity which merges into the cervical canal. I cannot explain the pathology of the condition of this structure rendering it so hyperæsthetic. I know, however, that this morbid sensibility can be overcome by the passage of metallic bougies, and it is the consideration of this which is before us.

As you are all aware this is not a novel treatment. In the year 1826 Dr. John Mackintosh, Lecturer on the Practice of Physic in Edinburgh, meeting with a servant girl, æt. 22, the subject of amenorrhœa—or *emansio mensium*—for she had never menstruated, but who since sixteen years of age complained regularly every month of pains in the loins, together with a sense of weight and bearing down in the passages, latterly so severe as to incapacitate her for work; finding on examination that the os uteri was a mere depression, he conceived the idea of dilating the canal, and accordingly commencing with a small triangular probe, went on until he succeeded in introducing a No. 6 male bougie to the fundus of the uterus; the result was that menstruation was immediately established, and proceeded regularly without pain or inconvenience.

As there is no record of swelling before the operation, or escape of retained menstrual fluid subsequently, no better evidence could be brought forward against the obstructive theory. No doubt the probing acted as a stimulus to the uterus, resulting in the menstrual discharge.

This case, however, suggested to Dr. Mackintosh the use of metallic bougies for the treatment of dismenorrhœa, still entertaining the idea of obstruction, although he says:—"I am far from alleging that dysmenorrhœa is always produced by a small os uteri, on the contrary, I believe it may occasionally depend on inflammation of the lining membrane of the uterus, as well as on inflammation in the substance of the cervix uteri, and on the encroachment of tumours diminishing the calibre of the passage through the cervix."

Dr. Mackintosh proceeds to say that he employed the dilators in twenty cases of dysmenorrhœa, permanently curing eighteen; ten only of these were married, living with their husbands, all were sterile, and seven subsequently fell with child. This is the statement made in 1839. Since that time he had tried the practice in seven other cases after all other means had failed; in one of the seven only did it fail, the other six were completely and permanently cured, and four of these bore children.

It appears, therefore, that out of seventeen married women, eleven, who were previously barren, became pregnant. Several of the cases are published *in extenso*; one woman had been married between seven and eight years, another three years, and so on.

He says:—"The instruments employed to produce the dilatation are the common metallic bougies, of different sizes, from that of the ordinary silver probe to No. 14. The operation is performed (the patient lying in the position in which women are usually delivered in this country) by introducing the index finger of the left hand till it reaches the os uteri for the purpose of directing the instrument to the part, which is then to be gently insinuated by a rotatory motion till it arrives at the fundus of the uterus. *Much force ought not to be employed.*"

With regard to the length of time after the dilatation that impregnation took place, "three months afterwards" is mentioned in one case, and "some months afterwards" in another.

"In speaking of dilatation of the os and cervix uteri as a means of cure for dysmenorrhœa, Dr. Simpson pointed out the results of this practice in the hands of the late Dr. Mackintosh in the cure of dysmenorrhœa and sterility connected with normal and inflammatory strictures of the os uteri. His own results had not been so successful as those of Dr. Mackintosh, but he had now seen a considerable number of severe cases in which dysmenorrhœa, that had previously resisted all other kinds of treatment, had at once yielded to the mechanical dilatation. Dr. Simpson had found the stricture occasionally at the os internum or opening between the cavity of the cervix and body, and not at the os tincæ. Dr. Mackintosh had effected the dilatation by long straight bougies of different sizes. Dr. Simpson had found them more easily used when slightly curved. Latterly, Dr. Simpson had in his practice thrown them aside, and used permanent bougies or stems made of Berlin silver, and he considered them far preferable."

I have treated altogether ten cases of dysmenorrhœa in married women in whom pregnancy had not occurred.

In one of these the dysmenorrhœa was much diminished during the two periods which followed the dilatation. It then returned as badly as ever, and I decided to introduce a silver stem, and leave it in the uterus. She has been cured of her dysmenorrhœa, but pregnancy has not followed. It is only fair to state, however, that her husband is seventy years of age.

In four of the other cases the dysmenorrhœa was cured, but as far as is known pregnancy has not resulted. One, a hospital case, has been lost sight of. Another had her cervix divided by the metrotome before coming under my treatment. In five of the ten patients pregnancy occurred.

Notwithstanding the severe criticism of Marion Sims, and its acceptance by other distinguished gynæcologists, I venture to say that the method properly conducted is not a dangerous one—is, indeed, safer than any other. The first bougie passed should be a small one, and there should not be sufficient difference between the size of it and that which follows to cause a splitting of the cervical canal, an occurrence which is likely to arise under other circumstances; the dilatation should, therefore, be performed at one time, and not on successive days, as hitherto recommended.

I do not mean to assert that dilatation by bougies is always successful; if it fail, other modes of treatment may be tried, the insertion of a silver stem, for instance, or division of the cervix. But I look upon these as more complicated and attended by more risk. I have seen so much harm follow the careless introduction of stem pessaries that I never venture to insert one without keeping the patient at absolute rest for two or three days afterwards, whereas the process of dilatation can be conducted with safety in one's own consulting room, the patient walking away and returning home afterwards. And, with regard to incisions, I was in the habit of just nicking the sides of the external os with the puncturing lancet in the out-patient room, until several cases of inflammatory mischief causing fixation of the uterus followed, and I was compelled to discontinue the practice. My cases show that a very small amount of dilatation is necessary; in three out of the five cases two bougies only, Nos. 7 and 8, having been passed—and an important point from my observations, upon which I would lay stress, is that the time selected for performing the dilatation should be within a week or ten days after a monthly period, and not close upon an approaching one.

The points I would place before the Society are:

1st. *How do the dilators act in relieving the dysmenorrhœa?* I have already suggested that the pain seems to be due to spasm, which at the approach of the menstrual flux seizes upon the uterus, the endometrium of which is in a state of hyperæsthesia. The contact of a foreign body with this seems at first to increase its morbid sensibility, but as the structure gets accustomed to its presence its sensibility becomes blunted, and the spasm subsides. The impression left upon the endometrium after the withdrawal of the bougies is such as to render it less sensitive and liable to spasm. How is this explained?

2ndly. *The dysmenorrhœa being got rid of, pregnancy occurs.* In none of my cases was there either stenosis or constriction of the canal by acute flexion. The theory, therefore, of permanent constriction being discarded, in what did the impediment lay? Was it a spasmodic constriction causing ejectment of the semen? It is possible that in Case 2 it might have been due to a large plug of coagulated mucus, for such was observed to come away during the dilatation, and pregnancy occurred immediately afterwards. But in none of the other cases was this noticed, and in three of them pregnancy did not ensue for a period of from two to four months. The idea of obstruction from a plug of mucus, therefore, must be rejected.—*Obstetrical Transactions*, 1881, *p.* 277.

85.—HYPERPYREXIA AFTER "LISTERIAN" OVARIOTOMY.

By G. Granville Bantock, M.D., F.R.C.S.Ed., Surgeon to the Samaritan Free Hospital for Women and Children.

My object in the present paper is not to treat the general question, but to limit myself to an examination of the claim that has been set up, that the chief characteristic of Listerism is an absence of pyrexia. It is this claim that I venture to dispute. The evidence that I have to bring forward on my adverse view of the subject comes first from my own experience and secondly from reports published by other operators.

1st. So far as my own practice is concerned the fact is this: as regards temperature the Listerian method, in a series of thirty-six cases, shows in its favour only a difference of four tenths of a degree, taking the average of the first four days, over another series of the same number done previously. Before the adoption of Listerism I had performed ovariotomy in thirty cases. I then operated six times according to Lister's method, but using thymol instead of carbolic acid. As this substance has been rejected as a germicide I may add these six to the preceding thirty cases, making up my first series to thirty-six. I then began Listerism pure and simple, with the exception that I did not use the "protective," preferring the dry dressing, which in one form or other has since become so general even among Listerian ovariotomists. Thus I continued up to my seventy-second case. What then happened, as I shall have to relate, forced me to reconsider the question. By this coincidence we have an opportunity of comparing the results of the two different systems of treatment in an equal number of cases. Of the first set of cases twenty-seven recovered. Of the second twenty-eight. The average temperature for the first four days was, in the former 100·3°, and in the latter 99·9°, a difference, as already stated, of only four tenths of a degree. But the most striking fact is that the lowest temperature is found in one of the non-Listerian cases, in which it did not exceed 99·6°. In every case of the second series the temperature reached the 100th degree. For the purpose of more exact comparison I have been able to bring together four sets of cases, of two each as nearly alike as possible on all points affecting the comparison. In the first pair, as is shown by the chart, the advantage is on the side of the non-Listerian by more than two degrees, while in the other three the advantage is only slightly on the same side. Of the non-Listerian cases the highest temperature was reached in one of the clamp cases and in a drainage case, in both of which it rose to 103·6°. But this compares favourably with the last four of the Listerian, in one of which it rose to 107·2°. The slight difference of only four tenths of a degree in the two series of cases may be accounted for by my want of experience in the earlier series, and by their more severe character. Thus, ten of the first series required drainage, only eight in the second. But this indicates more than meets the eye, for in the first series I was not so fully aware of the value of the drainage tube, and did not resort to it with the same frequency as in the second. Again, in the first series there were three clamp cases, in all of which there was high temperature, two requiring the ice-cap. The whole of the second series were treated with the ligature. Thus, in my Listerian practice it must be admitted that the claim for exemption from pyrexia cannot be sustained.

2nd. As for the evidence of others, no one has taken up Listerism more zealously than Volkmann, and the result is that he has not only encountered a state of post-Listerian pyrexia, but has admitted its occurrence by giving it the distinctive name of "aseptic fever." Then again Thiersch, while believing the theory of Listerism to be perfect, met with so much irritation from the use of the carbolic acid that he felt obliged to substitute salicylic acid. Keith, whose authority no one can question, in his paper on "Ovariotomy before and after Antiseptics," tells us that "carefully prepared tables of temperatures of the two sets of cases show very little difference. There was, as a rule, the same moderate rise of temperature, up to eight or ten hours after operation—more marked, *perhaps*, in both sets of cases in young subjects, especially if in too good condition; then a fall by next morning, and again a rise in the evening to about thirty-four hours after operation." Then he goes on in these words:—"In three cases (Listerian) the temperatures were the highest I have ever seen a few hours after ovariotomy. In one it rose to 104°, but was down by next morning. In another five hours after operation, it was 106·2°. In another, 105·5° eight hours after." The two latter "were long operations, and there was long exposure of intestine and mesentery to the action of the spray. Now, I have *rarely*, not more than twice, seen a temperature of 103° in the evening of the operation in any case before antiseptics." "I have rarely met with high temperatures in ordinary *operations*." "I had never, before antiseptics, found it necessary to use ice

;o the head to bring down fever in the first days after operation. The ice-cap was only used once in a case of acute septicæmia, and the temperature remained unaffected. Indeed, in all my cases before the spray not more than five or six pounds of ice were got for the whole number, and the most of that was wasted."

All this evidence, derived from my own practice, and from the experience of others, is in direct opposition to the Listerian maxim that there can be no fever after antiseptic operation. In the discussion already referred to, Mr. MacCormac remarked that "after antiseptic operations there was only a slight (if any) rise of temperature, and then attributable to other causes." Mr. Wells stated that "his general impression of the result of antiseptics in subduing pyrexia was opposed to Mr. Tait's experience." And Mr. Thornton said that "as a rule there is no fever at all after antiseptic ovariotomy."

How is this conflict of evidence to be accounted for? Is it because the advocates of Listerism do not fully recognise the facts that present themselves? Or is it that they allow theoretical "general impressions" to modify their observations?

Having, as I believe, established my assertion that pyrexia in various degrees does follow Listerian ovariotomy, it is worth while to inquire how this is to be accounted for by those who have given their attention to the subject. Keith attributes hyperpyrexia to operating on women overfed, or in too full health, with small tumours, or to imperfect cleaning, or not draining of the abdomen. Yet he is "unable to account for the rapid rise of temperature in two" of the cases already quoted. Thornton, in a recent paper, "On the causes of Occasional High Temperature after Antiseptic Ovariotomy," accounts for it by "reactive, nervous temperament, metrostaxis from irritation of ovarian nerves, and nerve-tension from too tight ligature of the pedicle," and gives as the most serious causes "congestion of the kidneys, deranged circulation from removal of a large tumour with little hemorrhage, local peritonitis, sweat rash, and failure in perfect aseptic operation." While admitting the efficacy of some of these conditions in producing hyperpyrexia, I maintain that one of the most important and frequently acting has been overlooked. I refer to the action of carbolic acid, either as a local irritant, or as a poison.

I purpose to treat these two conditions separately, and first carbolic acid as a local irritant.

There can be no doubt as to its action in this way, and it will, I am sure, be thought sufficient if I quote Mr. Lister alone on this subject, not that there is a scarcity of evidence.

This evidence is contained in the single word "protective," the name given to the material which he interposes between the wound and the carbolic acid. In a clinical lecture, delivered at King's College Hospital on Nov. 24th last, he devoted some time to a repetition of his views on this subject, and he took pains to show that a most elegant preparation called hyaloderma, which has come over to us from Russia, lacked the one essential property of the "protective," inasmuch as it allowed the antiseptic to pass through and thus gain access to the wound. The effect of such success would be irritation of the wound and consequent elevation of temperature. Let me add to this the frequently observed action on the skin, under the gauze, resulting in a copious pustular rash.

Second, carbolic acid as a poison.

It has been long known that carbolic acid produces, in addition to its direct local caustic action, profound constitutional disturbance which may prove fatal without any evidence of local action. The first sign of this constitutional action that has been observed is the well known dark discoloration of the urine. In the earlier days of the use of carbolic acid as an external application, this effect was frequently noticed by surgeons, until the strength of the solution was reduced. It was not long before constitutional symptoms were observed, such as great depression, loss of appetite, &c., and these symptoms were not necessarily in proportion to the discoloration of the urine.

It appears that carbolic acid may be introduced into the circulation in a variety of ways. Thus, it may gain access by inhalation, by absorption through the skin and mucous membranes, through recent and through granulating wounds, and through the peritoneum and serous membranes.

So long ago as 1870, Dr. Lightfoot is found referring to a case of death from inhalation of carbolic acid for toothache.

In the "Berliner Klinische Wochenschrift" for Oct. 6, 1879, a case is recorded by Dr. Hannhorst in which the application of a 15 per cent. solution of carbolic acid, by means of pads of cotton wool, to a commencing phlegmon on the arm (in the person of a nurse) caused the most grave symptoms in a few minutes. The patient lost consciousness, she threw herself about in bed, her face was flushed, pupils contracted, pulse slow, and breathing laboured. A 3 per cent. solution was substituted. In an hour the patient had recovered consciousness, and she was able to explain that, immediately after the application of the dressing she had felt a pain in her arm, and then lost consciousness. She now complained of fatigue and frontal headache. There was complete suppression of urine. Here then the absorption was through the unbroken skin. In connection with this I may mention another fact communicated to me by a distinguished surgeon, viz., that after a Listerian operation he invariably feels a pain, for some hours, up the arms. To this class also belongs a case incidentally noticed by Mr. Lister, in which the application of spray and gauze, after amputation of the breast, repeatedly, *i.e.*, with each renewal of the dressing, produced symptoms of carbolic acid poisoning, such as general debility, loss of appetite, and other symptoms.

As to its absorption through the mucous membrane, I need only allude to the numerous cases proving fatal when taken by the stomach, and to a case (also referred to by Dr. Lightfoot) which occurred at the Worcester Infirmary, in which death resulted in twenty minutes from the administration of a carbolic enema.

Carbolic acid may also be introduced into the system through a flesh wound. Prof. Parkes, of Chicago, related to me the particulars of a case of amputation of both legs, in which the patient suffered from great prostration, hyperpyrexia, and suppression of urine. At a later stage the urine yielded hyaline casts, albumen, and indican in great quantity. Mr. Allingham informs me that the injection of a 5 per cent solution of carbolic acid in the case of a patient on whom he had just performed excision of the rectum proved fatal in a few hours with the usual symptoms.

In my own practice of ovariotomy this proposition is also established by several cases. But I need only bring forward three to show that the carbolic acid, being so introduced, may fairly be assigned as the cause of the hyperpyrexia which characterised them.

On March 26, 1879, I operated upon a woman, æt. 38, removing both ovaries. There were extensive adhesions to the omentum and to the right half of the pelvis, including the back of the uterus, which was much lacerated, and required several ligatures. To provide for the escape of some blood which continued to ooze after I had secured all the bleeding points that could be brought into view, I put in a drainage-tube. The operation lasted about an hour and a half. Two hours afterwards, two ounces of urine were drawn off, but thence to the end of the case not more than two drachms were obtained. Five hours after operation the pulse was between 150 and 160, and very small, temperature 99·4°. At the end of twelve hours the temperature was 103·3°, and in four hours more 103·8°.

The pulse was then 170 to 180 and very difficult to count, and the respirations were about 30. Stimulants were freely administered, but she gradually sank and died in twenty-seven hours. Post-mortem examination showed the kidneys enormously enlarged and deeply congested. There could be no doubt that the immediate cause of death here was the acute nephritis. But what was the cause of the nephritis? This was soon to be revealed, and was made clear fifteen days later by the case I now proceed to relate more in detail.

The salient points are these:—That the patient, æt. 55, had previous to operation been somewhat reduced by repeated tappings, which yielded 417 pints of fluid within eighteen months; that there was a large extent of raw surface from the rupture of adhesions; that the temperature rose to 107·2° within thirteen hours of the operation; and that there was complete suppression of urine for twelve hours, followed by symptoms of acute nephritis. Recovery was due to the use of the wet pack.

Numerous are the instances of poisoning through a granulating surface. Such cases are met with in great variety. Of these, perhaps the most interesting, as far as I know, is that reported by Dr. De Agostini in the 'Gazzetta degli Ospitali" for February 1st, 1880. A man, æt. 44, had a perinephritic abscess opened under spray, and the gauze dressing was moistened with a 2 per cent. solution of carbolic acid. After the operation the temperature rapidly fell, and in a few hours became normal. Next day the abscess cavity was twice washed out with a 1 per cent. solution until the fluid returned clear. This process was repeated daily. On the fourth day the fever returned, the temperature rising to 101·3°, and for several days it went on gradually mounting higher and higher, until on the tenth it reached 102·5°. At this time the wound looked quite healthy, and in the hope of arresting the fever the irrigations were ordered to be more frequently and diligently employed. The same night the patient was seized with a violent attack of dyspnœa, with abundant serous expectoration, amounting to half a litre in the night, the pulse was small, there was great prostration of strength, for a time there was tracheal rattle, and the patient was considered to be in imminent danger. Next day this grave condition prevented the renewal of the dressings more than once, and a smallar quantity of the solution was used. At noon the temperature was 103·2°. On the following morning he was much better; temperature normal between eight and nine, but in the evening it rose to 101·4°. Two days afterwards, after examination of the chest, some limpid fluid was drawn from the left pleural cavity by means of Pravaz's exploring needle, with great relief to the symptoms. For a week the fever observed a remittent type, although the wound gradually progressed, the pus diminished in quantity, and the chest symptoms disappeared. The dressing was still changed once a day, but the amount of solution used was iminished, partly because the cavity was contracting and artly because the patient asserted that he felt worse after each ressing. The temperature then varied between 99·5° in the norning and 103·1° in the evening. Large doses of quinine requently repeated had no effect. At last, suspecting the arbolic acid, Dr. De Agostini ordered salicylic acid to be ubstituted. In a few hours the temperature fell, and so apidly that in thirty-six hours it was only 97·5°. In twelve lays more the wound had healed, and the patient returned to his ccupation. A long time afterwards Dr. De Agostini was nformed that when the symptoms were at their worst, the rine was of a dark olive colour.

Numerous observers have called attention to the fact that he young are peculiarly susceptible of the action of carbolic cid applied externally. So impressed was Langenbeck with his peculiarity that, after witnessing two deaths after operaions of no importance, he ceased to employ the spray in such ubjects. Dr. Hunter, of Linlithgow, informs me that he has requently observed serious carbolic intoxication from the pplication of a carbolised lotion to inflamed and suppurating accine pustules. And Mr. Thomas Smith, in an article in the 'St. Bartholomew's Hospital Reports,' also refers to this usceptibility. It is probably due, in some measure, to the outh of my patient that I again encountered an instance of narked intoxication in the following case:—

On April 3rd last I removed a large ovarian tumour, weighng over thirty-three pounds, from a young girl, æt. 16, who had een tapped a few weeks previously. The adhesions were umerous and extensive to parietes, omentum, and floor of elvis, and the oozing of bloody serum was so abundant that had to have recourse to the drainage tube. The strength of he solution and spray was only one in sixty. Nine hours after he operation the temperature had risen to 102·8° and the pulse o 142, and the ice-cap was applied. Suffice it to say under his head that the temperature rose to 103·2° within the next our, and then fell gradually until at the end of twelve hours t was only 99·2° (pulse 88), and the ice-cap was taken off, and hat with fluctuations of varying intensity, once reaching 101·8°, t did not attain the normal standard till six days after. *wenty ounces* of bloody serum, in gradually diminishing quanities, were obtained from the tube in the course of the twentyight and half hours it was in use, and as evidence of the omplete absence of septic fermentation I may state that the uid removed on the last occasion, at once transferred to a ottle, still remains free from change: nor did the microscope how any bacteria. I mention this for the purpose of excludng the idea of septic mischief as the cause of the hyperpyrexia.

Four hours after the operation twenty-five ounces of urine was drawn off, pale and slightly muddy, and containing a litt albumen. On adding to it a solution of chloride of barium th only change was the slightest possible milkiness. Fearing that I had omitted something from the test, which I applied from memory, I obtained some urine from another patient no exposed to the action of carbolic acid. The chloride yielded a abundant and dense precipitate. Four hours later seven ounces of urine were drawn off, and this gave even less precipitate producing only the faintest possible opalescence. After fou hours more six ounces of urine were got. This gave a decide precipitate and more albumen, and four hours later still th urine (six ounces) yielded a copious precipitate, though not s much as the normal urine. After this it was not tested wit the chloride. On the fifth day the urine contained as much a one third of albumen. On this day the temperature range between 99·6° and 101·2°, and she was ordered ten-grain dose of cream of tartar (pot. tart. acid.) three times a day. Die consisted of milk and barley water. On the seventh day th albumen was still very abundant, and I prescribed tr. fe mur. ♏iiss, tr. digitalis ♏iiss, extr. ergot. liq. ♏v, ever four hours. On the following day the albumen was reduced t about one twentieth, and three days later it had quite dis appeared. The patient went home on the twenty-sixth day.

It only remains for me to add that I have now proved tha the claim that has been set up for Listerism cannot be sustaine (in preventing pyrexia after operations); that carbolic acid ma be introduced into the circulation in poisonous quantity b means of this method; that when so introduced it manifests i presence by producing a state of hyperpyrexia, and that thus i actually produces too often what it was intended to preven And the practical result of all this, so far as I am personall concerned, is that, to meet the evil which to me is so formid able, I have gradually diluted my spray and solutions so as t reduce the whole question to one of *cleanliness*, which, after al is the true secret and merit of Listerism, and I am happy t say with the effect of very greatly adding to the success of m ovariotomy work.—*Medico-Chirurgical Transactions*, 1881, *p.* 10

86.—ANTISEPTIC OVARIOTOMY, WITH REMARKS ON RECENT IMPROVEMENTS IN THE OPERATION.

By T. Spencer Wells, F.R.C.S., Surgeon to the Queen Household; and to the Samaritan Hospital.

The total number of deaths among the 1000 patients (in th number of cases of completed ovariotomy in my own practic up to June 1880) has been 231, while 769 have recovered.

very large proportion of those who recovered have remained n good health. Many have had one or more children since the operation. In a very few the ovary not removed at the irst operation has become diseased and has been removed, generally with success, by a second ovariotomy. In some cases the patients have died at an advanced age, or of some disease quite unconnected with the operation; death from a recurrence of ovarian disease, or from malignant disease where malignancy of the ovarian growth had been observed or suspected, having been quite exceptional.

With the beginning of 1878, and the limitation of my pracice to private patients, must also be associated the commencement of the testing of what are called "antiseptic precauions," or the details of the "Listerian method." Long beore Mr. Lister had tried any of his methods, indeed, from the very beginning of my practice of ovariotomy, I had insisted upon all possible care in protecting patients before, during, and after operation from all the known causes of excessive mortality, and I took unusual precautions against any risk of contagious or infectious disease being communicated to a patient, and against the entrance from without, or the development from within, of anything which could set up traumatic fever or blood-poisoning. I contended that obstetrics and operative gynæcology should seldom be permitted in the same building, or by the same surgeon in private practice; and that such an operation as ovariotomy should never be performed when paients with uterine cancer, or offensive discharges of any kind, may pollute the place. In 1875, a separate branch of the Samarian Hospital was opened, and since that year the surgical wards have been much freer from such sources of danger. The good ffects of this change were noted before other antiseptic meaures were insisted on. And cleansing or purification of the ward or room, of everything about the operating table and bedding, of the patient herself and the parts near the seat of peration, of the surgeon, assistants, and nurses, and of all the nstruments, sponges, and water used, had gradually become more complete before carbolic acid was used, or any antiseptic precaution added to those adopted before 1878. But in no series of 100 cases had I succeeded in reducing the mortality elow 17. Then in the last series of 100 cases, all done antiseptically, the mortality fell to 11. Of the 13 cases, all antiseptic, from 888 to 900, only 1 died. So that the deaths in 113 ases since adopting more complete antiseptic precautions were 2, a mortality of 10·6 per cent. If I may be permitted to add 1 cases, done since the completion of the thousand, with nly 1 death, the total number of antiseptic ovariotomy cases n my practice amounts to 134, with 13 deaths, a mortality of ·7 per cent.

Before inquiring how far this smaller mortality may b due to the additional antiseptic precautions, or to oth causes, I will explain precisely what the additions or chang have been.

As the material for tying vessels and uniting the wound, t same pure twisted silk, unmixed with any vegetable fibr which I have trusted to for about twenty years, has been use I have never tried catgut; and after trial, have abandon whipcord, hempen ligatures, silver iron and platinum wi horsehair and other materials. Various forms of quilled a twisted sutures have also been tried and abandoned. B since 1878, all the silk for ligatures and sutures has bee soaked before use in a five per cent. solution of carbolic acid phenol. Dry dressing of the wound has been continued; but i place of the pads formerly used, of 5 per cent. of oil of tar wi 95 per cent. of chalk, either thymol gauze, or cotton pa charged with borax or phenol have been used. These are mo comfortable to the patient, and are better absorbents of moi ture. As a rule, they are not touched before the seventh eighth day, when the sutures are removed, and the wound almost invariably found to be completely united.

The two most important additions to previous antisept precautions are, first, carbolising the sponges and instrument and secondly, the use of the spray. I have long insiste on the great importance of always using perfectly pu sponges, and I believe this object is more perfectly attaine by soaking them in carbolised solution after washing, than b washing alone.

After an operation, I continue my old plan of keeping th cleansed sponges in a weak solution of sulphurous acid. An during the operation, in addition to washing in pure wate every sponge before use is wetted with a 2 to 3 per cen solution of carbolic acid or absolute phenol. Soft clean line cloths, wetted with a warm solution of phenol, may be use to lessen the number of sponges required; and nurses must b cautioned not to put any of the soiled sponges into the solutio after until they have been washed, otherwise albumen may be coagulated as to prevent thorough cleansing. As nurses ofte fall into this error, it is well to have two or three different se of sponges, all carefully numbered, kept separate for the suc cessive steps of the operation.

Nearly all the instruments used in ovariotomy may be pro tected from rust by a coating of nickel. They are then mo easily and thoroughly cleaned after use, and the cleaned instru ments should be placed before, and replaced during, the opera tion in trays or dishes filled with a warm solution of *phenol*.

These additional precautions as to sponges, silk, and instru nents, I believe to be really important. I feel still doubtful about the spray. In prolonged operations, I have had reason to fear that its chilling effect upon the patients has been injurious. But I have never once seen any other ill effect which I could attribute to it, nor anything like carbolic poisoning. The misty cloud occasionally obscures the field of operation, but not to any serious extent, and it is always easy to protect the peritoneal cavity by a large warm sponge against the continued action of the spray. After a few trials I gave up thymol spray as useless, and for more than a year past have used a spray of absolute phenol of a strength of one in forty. And this I continue to use, believing it to be safer than the irrigation or sponging proposed as substitutes, although I fully admit that we require a far greater number of trustworthy experiments, or of comparative observations made under similar conditions with and without spray than have yet been made known, before we can receive any satisfactory answer to the questions whether carbolised vapour or air can destroy or render innocuous, infective, or putrefactive substances or germs floating in the air; or what is the share which the spray, among other additional antiseptic precautions, has had in obtaining the better results which have undoubtedly accompanied their combined employment.

On carefully going over the notes of all the cases to ascertain if the smaller mortality in those treated antiseptically could be due to any other cause, the only modification in the mode of operation which calls for further remark is the very much more frequent, almost constant, employment of the *intra*-peritoneal treatment of the pedicle since the trial of the antiseptic system was begun. But before that time, the extra-peritoneal treatment had been by far the more successful in my practice. When comparing the results of the two methods at the College of Surgeons in June 1878, I showed that of 627 *extra*-peritoneal cases, 130 had died, or 20·73 per cent., while of 157 *intra*-peritoneal cases, 60 died, or 38·2 per cent., the mortality with the ligature having been nearly double that of the clamp. I am quite sure that, as has been suggested, the *extra*-peritoneal did not represent the simple, and the *intra*-peritoneal the complicated cases. The difference was simply that of long or short pedicle. Whenever the pedicle was long enough, I used to employ a clamp whatever might be the complications of the case; and in short pedicles I used the ligature or cautery, whether the case was otherwise simple or the reverse. To my mind, one great merit of the antiseptic system is that it has made the *intra*-peritoneal method, which was formerly the less, now the more successful method of dealing with the pedicle. Formerly, septic changes, which are now scarcely ever observed, frequently took place in or about the tied pedicle, and the many disadvantages of the *extra*-peritoneal method, which were only counter-balanced by its greater success, have no longer to be endured.

Another great gain from the antiseptic system is that drainage of the peritoneal cavity is now scarcely ever necessary. In the paper which I brought before this Society on completing 800 cases, I contended that drainage should only be an exceptional practice. But I did not then imagine that it could be almost entirely discarded. I can now say that I have not drained one case in which antiseptic precautions have been taken; and on looking back I cannot believe that there are more than two in which, if a drainage tube had been used, it could have been useful. The simple explanation is, that the mixture of blood, other fluids, and air left in the peritoneal cavity, or oozing into it after operation, formerly went through putrefactive changes, and, if not drained off, produced septicæmia, whereas now no putrefaction takes place, and absorption is quite harmless.

My own experience has not only convinced me that by the use of antiseptics—especially of phenol—the success of ovariotomy has been remarkably increased, that a much smaller proportion of deaths to recoveries has been obtained; but, further, that those who have recovered have suffered much less from fever, while convalescence has been more rapid than it used to be. Formerly, temperatures of 100° to 103° were usual, and 104° to 107° not very uncommon. And the head was cooled by ice in at least half the cases. Now, cold to the head is scarcely ever thought of, certainly not used in one case in twenty, and a temperature of 102° is rare. Recovery with a temperature which never rises above 100° is the rule.

It would occupy too much time if, in this evening's meeting, I were to review the progress of ovariotomy from the revival of the operation which followed the discussion on my first five cases in this room twenty years ago. I must content myself with the knowledge that an operation which was then a rare and exceptional event in this country, and still more so abroad, which had been discredited in more than one important discussion in this Society, and which was then truly regarded as one of the most fatal of surgical operations, is now so frequently performed that several surgeons at home and abroad number their cases by the hundred, that sufficiently full details of more than 3000 cases can be grouped together for study, and that an average mortality of about 10 per cent. has already been attained by several operators.

Surely these facts should encourage the hope that in the future, even this small mortality will be considerably smaller. —*Medico-Chirurgical Transactions*, 1881, *p*. 167.

87.—OPERATIONS FOR REMOVAL OF UTERINE TUMOURS.

By J. Knowsley Thornton, M.B., C.M., Surgeon to the Samaritan Free Hospital.

These operations are yet in their infancy, and we have everything to learn, but I think those recorded in my table teach some useful lessons.

There are three cases of operation for the removal of fibrocystic tumours of the uterus, and in one of them the left ovary was also removed; all recovered.

There are five cases in which uterine outgrowths were removed with or without one ovary; four out of the five recovered. Thus, we have seven recoveries in eight operations, but mark the fact that in none of them was the uterine cavity opened, and hence it was possible to complete really aseptic operations. In the fatal cases (three of the antiseptic series), the pregnancy and other complications fully detailed in the 'Transactions of the Obstetrical Society,' vol. xxi, amply explain the fatal result, which was immediately due to obstruction of intestine.

In one other case of the antiseptic series an outgrowth was removed, but the cavity of the uterus was opened, and the opened portion was included in the blades of a Wells' ovariotomy clamp; this portion sloughed, and the patient died of septicæmia.

In three cases hysterectomy was performed; and in two of them both ovaries were removed along with the supra-vaginal portion of the uterus. One case died of shock almost immediately; another, in which I trusted to the cautery, died in eight hours of hemorrhage; and the third recovered without a single bad symptom, and is perfectly strong and well now. This last case, together with a similar one successfully operated upon before I adopted Lister's method, show that there is not *necessarily* any great shock in this formidable operation.

What we have to fear in these cases, and in those in which the cavity of the uterus is opened in removing fibro-cysts or solid outgrowths, is *sepsis*. What we have to find out is how to render these operations really *aseptic*, or how to deal with the divided uterine wall in such a way as to render the septic material least dangerous. Suture of the peritoneal edges of the uterine wound, so as at once to close the peritoneal cavity, the raw surfaces being left without suture towards the interior of the uterine cavity, in order that discharges may escape by the cervical canal, seems to answer in some cases; but this method has one great disadvantage.

The ligatured mouths of the vessels and portions of half strangled uterine tissue are extruded from our aseptic area, and we know from operations in the interior of the uterus for the removal of polypi, &c., that stumps in the cavity, however treated, are very apt to lead to septicæmia.

I prefer the Wells' clamp to anything else that I have used or seen used for complete hysterectomy; but unfortunately it leaves us also exposed to the risk of septicæmia, and can only be applied in a small number of cases in which the supra-vaginal portion of the cervix is not much enlarged. It has also the disadvantage of much prolonging convalescence and leaving a nasty sore, and eventually a weak place in the cicatrix.

I am firmly convinced, from what I have seen of this branch of uterine surgery since the introduction of antiseptics, that we only want a surely aseptic method to render complete and partial removal of the uterus as little fatal as ovariotomy.

The remaining cases in the table call for no special comment, one being merely exploratory, one a removal of a small piece of adherent omentum which was causing pain, and the other two having been already separately published.

The great lessons I have learnt while performing these one hundred and seventy-two operations may be briefly stated:—

1. In the simple cases the patients recover under Lister's method with a certainty previously unknown.

2. There is less fever, and convalescence is more rapid than under the old method.

3. The success obtained in the more complicated cases is in proportion to the exactness with which the antiseptic method can be applied to the individual cases.

4. The accidents and complications occasionally following operations, such as hemorrhage, for example, are more easily overcome in cases which are strictly aseptic.

5. There are difficulties, and even dangers, in the application of the method, and the more experience the individual surgeon has in it, the more readily he foresees and avoids these, and the more complete becomes his success in applying it.

I would say, in conclusion, that great as I believe the debt to be which peritoneal surgery owes and will owe to Mr. Lister, I am far from forgetting that it owes a still larger debt to Mr. Spencer Wells; without his untiring energy and success, we should to-day have been as backward in all peritoneal surgery as, I fear, we still are in one special branch of it, viz., the treatment of uterine tumours. To Mr. Wells the glory of ovariotomy must ever be, but to some fortunate follower may still belong the credit of placing hysterectomy and its modifications in an equal position, and whoever he may be his labours have already been lightened for him by the application of Lister's method to abdominal surgery. — *Medico-Chirurgical Transactions*, 1881, *p.* 148.

88.—ON THE DEVELOPMENT OF MALIGNANT FROM NON-MALIGNANT TUMOURS OF THE UTERUS.

By William Maslovsky, M.D. (From the Laboratory of Prof. Slawjansky's Gynæcological Clinic, St. Petersburg.)

For the last ten years it has been a question in gynæcology whether malignant tumours of the epithelial type (carcinoma) can be developed from adenoma. During this time the possibility of such a change has been discussed by pathologists.

In the year 1873 Dr. Matthews Duncan and Prof. Slawjansky described a polypus removed in soft masses from the uterine cavity of a woman aged 52, whose hymen was entire. Considering the age of the patient, the general appearance, softness, and fragility of the tumour, Dr. Duncan thought it was malignant; but Prof. Slawjansky, after a careful microscopical examination, decided that it was non-malignant, an adenoma uteri polyposum hemorrhagicum.

Six weeks after the operation Dr. Duncan examined the patient on account of metrorrhagia, and found that the portio vaginalis uteri and upper part of the vagina was occupied by a growing tumour, which was easily torn, but did not bleed freely. In this case, therefore, there was recurrence of the adenoma.

Dr. Duncan relates another case occurring in his practice, of a woman rendered anæmic by metrorrhagia. After dilating the cervix, nodular masses were discovered, and removed with forceps. The microscopical examination by Dr. Hardie showed that the tumour differed but little from that described by Prof. Slawjansky. The points of difference are not mentioned. Dr. Duncan observes that the woman was not benefited by the operation. These cases raised the question of the development of malignant tumours from adenomata which were not malignant.

In the year 1876 Klebs suggested that carcinoma of the body of the uterus might be developed from polypous adenoma by proliferation of the glandular elements, but the occurrence was proved by the investigations of Breisky and J. Veit.

Breisky describes the case of a woman, aged 36, who suffered from metrorrhagia. The portio vaginalis was entire, and numerous polypi were attached to the mucous membrane of the uterine cavity. Portions of the polypi were removed and examined microscopically by Prof. Eppinger, who found them to be adenomata. About four months later, on account of repeated metrorrhagia, the cervix was again dilated, and portions of tumour removed. This time they were different from those formerly taken away, being softer and more fragile, and having the appearance of carcinoma. Prof. Eppinger's examination showed that the glands, which at the first examination contained only cylindrical epithelium, were now lined by flat epithelium with carcinomatous prolongations into the underlying tissues.

Veit describes the case of a woman, aged 57, suffering from metrorrhagia. A polypus of soft consistence was found in the dilated cervix, but on the next day, when it was to be operated on, it had disappeared, and its base, 23 centimetres in diameter, was found in the fundus. This was removed with a sharp spoon, and was about the size of a walnut. Microscopical preparations of the tumour showed some glands with a distinct lumen, and lined by cylindrical epithelium; others with distinct walls filled with epithelial cells of different forms and sizes, and amongst the glands, but apparently separated from them, masses of large epithelial cells. Veit considers that this case proves the development of carcinoma from adenoma, and that the carcinoma is formed, not from normal uterine glands, but from hypertrophic elements of adenoma. The investigations of Breisky and Veit thus confirm the anatomical transformation of adenoma into carcinoma, and establish two forms of carcinoma—the diffuse (Breisky), and the polypoid (Veit).

During the next four years nothing further was published on the subject. I was induced to take it up myself, by the rarity of the disease, and by the possibility of following on the same patient the various stages of the transformation of adenoma into carcinoma, and of establishing a differential diagnosis between them. I made use of pathological preparations of the uterus and of portions of tumours kindly supplied to me by Prof. Slawjansky from two of his cases.

On comparing the two cases we see that they present a compound form of tumour. On the one hand are adenomatous cavities with typically arranged cylindrical epithelium, and on the other cavities developed gradually from them, completely filled with cells, and sending prolongations of cells into the neighbouring tissues. The transition is effected by the multiplication of the mural or deep layer of cylindrical epithelium in two directions, inwards to choke up the cavities of the glands, and outwards to infiltrate the tissues.

The epithelium filling the lumen of the glands is either multiform (first case) or uniform (second case). The lining cylindrical epithelium in the choked glands maintains its radial arrangement. The epithelium spreading in the tissues either simply separates the fibres in the neighbourhood of the glands, or infiltrates the tissues diffusely, or extends along the lymphatics. Both the histological structure and the clinical course (dissemination and rapid recurrence in the second case) oblige us to acknowledge the malignant nature of the final tumour (adenoma malignum). In anatomical structure the tumour is analogous to the form of carcinoma of the large intestine described by Klebs under the name of epithelioma-cylindro-cellulare carcinomatosum. The above facts, establishing as they do a relationship between the benign and malignant polypous adenomata of the uterus, and explaining the origin of the latter, are of great practical importance in prognosis, for this must always depend upon the recognition of the malignity or harmlessness of the tumour.

The differential diagnosis of benign and malignant tumours depends upon their histological structure. In the benign the glands are abnormally large and contain a lumen which may be dilated to form a cyst, but all are lined with cylindrical epithelium. The epithelium is typically arranged,—that is to say, the cells are all placed at right angles to the gland wall, and are sharply separated from the subjacent tissues.

In the malignant, on the contrary, the epithelium is arranged in two layers, and may completely fill the cavity of the gland. The epithelium filling the cavities may be either uniform or multiform. In all cases the lower cylindrical layer of epithelium maintains its typical radial arrangement. In the last point the malignant adenomata differ from the carcinomata, in which the arrangement of the epithelium in the tissue spaces is different. The malignant character of the tumours is most marked when the epithelium is not separated from the underlying tissues—when it infiltrates them,—or spreads along the lymphatic vessels. In general it may be said that the uniform appearance of a harmless adenoma reassures the gynæcologist, while that of the malignant, with its epithelium proliferating and spreading in all directions, involuntarily excites him to adopt a timely and energetic treatment as the only means of saving his patient's life.—*Edinburgh Medical Journal, Jan.* 1882, *p.* 588.

89.—ON PAGET'S DISEASE OF THE NIPPLE.

By Robert Munro, M.A., M.D., Kilmarnock.

The clinical fact, first observed and announced by Sir James Paget, in 1874, of the frequency with which chronic eczema of the nipple was followed by mammary cancer, has since given rise to a very interesting discussion regarding the nature of the affection of the skin, and the pathological character of the breast-tumour subsequently developed. The enlargement of the area of observation by the publication of a considerable number of cases during the last few years has only strengthened the correctness of the original observation, but as to the incidental pathological questions or (what is of urgent practical importance) the diagnosis and treatment of the primary skin disease, unanimity has not yet been attained. At a meeting of the Royal Medical and Chirurgical Society, Jan. 11, 1876, Mr. Butlin, F.R.C.S., read a paper "On the minute anatomy of two breasts, the areolæ of which had been the seat of long standing eczema," in which he maintained, as the result of his histological observations, that the gland-ducts and acini were widely implicated in the morbid process as if by continuity from the cutaneous disease. The most significant pathological changes described consisted of distension and dilatation of the galactophorous ducts and acini with proliferation of their epithelium. No actual cancer cells were, however, observed. About a year afterwards, Jan. 23, 1877, Mr. Butlin contributed another paper to the same Society "On the minute anatomy of two cases of carcinoma of the breast preceded by eczema of the nipple." The pathological conditions here described were precisely similar to the former; but in addition, "there were in these cases fusion of the acini and ducts thus filled with proliferating epithelium into larger and irregular spaces, escape of the contents or growth of the contents into the surrounding tissues, producing the full formation of carcinoma." The general conclusion to which Mr. Butlin was thus led was, that cancer of the breast is essentially a disease of the epithelium of the mammary gland. In the discussion that ensued on the reading of Mr. Butlin's paper, Sir James Paget is represented as having said (see Lancet, Jan. 27, 1877) that "he could only speak of the clinical characters of these cases. He had no doubt of the frequency with which chronic eczema or any chronic irritative disease of the breast or nipple was followed by cancer. He had now seen some fifteen or twenty such cases, and regarded the sequence as so certain that he would venture to prophesy that if any irritative condition were present in the skin of the areola and nipple for a period of three or four years (the patient being of an age favourable to cancer), cancer of the breast would ensue. The cases described by Mr. Butlin were examples of 'eczema;' but he had met with two or three instances which might more fitly be termed 'psoriasis;' others in which only an excoriated surface was present; but all had the same issue—viz., the development of cancer, not in the skin, but in the substance of the gland." Mr. Butlin's views regarding the nature of cancer of this special organ, afford an easy transition to the more general proposition which many histologists hold—viz., that all varieties of cancer have an epithelial origin, and to this the discussion naturally drifted; but the problem lies outside the scope of my present inquiry.

A case of cancer of the breast at St. Bartholomew's Hospital, under Mr. Savory, is reported (Brit. Med. Journal, Dec. 15th,

1877) as being preceded by eczema of the nipple for sixteen months before the breast tumour was detected. At the Clinical Society, London, Oct. 24th, 1879, Mr. Lawson read notes of a case in which the eczema of the nipple had preceded the cancerous formation in the breast by a period of four years. In the discussion which ensued over this case, Dr. Thin expressed an opinion that the skin disease was not an *eczema*, but an inflammatory irritation spreading from the cancerous condition of the epithelial elements at the mouths of the galactophorous ducts—that in short the cancer preceded the eczema. Mr. H. Morris, M.A., F.R.C.S., opposed Dr. Thin's view on the ground that the diseased condition of the areola in two cases which he cited had existed for no less than six years—too long not to have preceded the cancer.

Shortly afterwards (Dec. 9, 1879) the discussion on this subject was reopened at the Royal Medical and Chirurgical Society, by the reading of two papers, one by Mr. Morris on "Two cases of carcinoma of the breast preceded by so-called eczema of the nipple and areola;" and the other by Dr. Thin, on "Eczema of the nipple and cancer of the breast; an inquiry into the nature and mutual relations of the morbid conditions which have been associated under these names."

The remarks made by the president, Mr. Erichsen, on this occasion (the substance of which I quote from the Brit. Med. Jour., Jan. 24, 1880), give a clear view of the issue and practical importance of the investigations made regarding the nature of the disease up to that date. He said that "the question brought forward was one of much interest, both clinically and therapeutically. As to the nature of the disease of the nipple, Sir James Paget, with characteristic caution, described it as 'so-called eczema,' declining to offer a decided opinion as to its nature. And when the results of the minute histological investigations made by Mr. Morris and Dr. Thin were considered, there was room for doubt as to its real character. Whatever was the nature of the eczematous affections of the nipple, they did not partake of the ordinary characters of the skin-cancers; but there was no question that they differed from common eczema. He could not but think that this condition of nipple, when associated with cancer, was something different from any known form of cutaneous disease. It had no doubt often been seen in connection with cancer, but its importance had not been recognised. As attention was first called to it by Sir James Paget, he would suggest for it the name 'Paget's Disease' of the nipple, as expressing its distinct existence and involving no theory. It was most important to determine its connection with cancer of the breast; whether it was independent of, or an evidence of the presence of cancer.

The great impediment in the surgical treatment of cancer was the difficulty of operating early enough to be sure of removing all the local disease. If 'Paget's Disease' were a necessary precursor of cancer, the natural inference would be, that every breast in which it appeared should be removed. But, as yet, it was scarcely possible to go so far as this, or to recognise eczema of the nipple as necessarily indicating the existence of cancer in the lacteal ducts. The relation of the so-called eczema to cancer was a subject which required most careful investigation."

The prominence thus given to "Paget's Disease" through the medium of the Medical Journals led to the publication of several well recognised cases throughout the country, notwithstanding that Dr. Thin, at the meeting of the Royal Medical and Chirurgical Society (Jan. 23, 1877), maintained that the supervention of cancer upon eczema of the nipple was unknown except in London. At the Glasgow Pathological and Clinical Society, May 10, 1880, Dr. R. W. Forrest showed a patient having "Scirrhus of the mamma with a small eczematous patch near the nipple, and which was interesting as being the first recorded case in which eczema was associated with scirrhus in the male breast." At the same time Dr. Foulis showed two specimens in which "Scirrhus of the mamma was associated with excoriation of the nipple, and in which the characters were not those of duct-cancer."

Meantime, Dr. Thin has not relaxed his histological investigations, and has lately published some additional facts in support of the theory that the breast-tumour in "Paget's Disease" is duct-cancer developed from the epithelium of the lactiferous ducts, and never true scirrhus or parenchymatous cancer, which is developed from the secreting epithelium of the acini; and that, from the very commencement, the disease in the epithelial element at the mouths of the ducts is of a cancerous nature which, in the course of time, spreads downwards to the gland, while it keeps up superficially this so-called eczema, or as he (Dr. Thin) proposes it should be called, "*Malignant papillary dermatitis.*" (See *Retrospect*, vol. 84, p. 312.)

It is quite clear, therefore, that in whatever aspect we view "Paget's Disease" of the nipple, its investigation is far from being exhausted, and that much more has to be done, both clinically and pathologically, before perfect unanimity can be attained regarding the important points at issue. There is one thing, however, which is of paramount importance to the medical practitioner, which brooks no delay, and which alone concerns suffering humanity, and that is the treatment of the disease. Here, however, the scientific specialist occupies a position scarcely above the platform of the empiric. The real difficulties, and consequently the gravest responsibility, occur at the very threshold of the inquiry. Actual experience of these difficulties drew my attention to the disease before I became conversant with its historical investigation, and it is with the hope that my observations, which are exclusively derived from the clinical standpoint of the general practitioner, may be of some practical utility, that I have thus ventured to bring them before the profession. The following cases are presently under my care:—

Case 1.—Mrs. B. A. (aged 58, mother of several children, youngest of whom is 16) consulted me at the beginning of the summer of 1879, about a slight excoriation of the nipple of the left breast, which had attracted her notice for a few weeks previously. Being the first time I had seen anything of the kind, I ordered a stimulating ointment composed of oxide of zinc, glycerine, and carbolic acid, but some weeks afterwards, seeing its still unhealthy-like appearance, I touched it with nitrate of silver, and followed up this application by simple water dressing. Subsequently I used several stimulating lotions as well as arsenic internally, but all to no purpose. I then recommended a consultation, and accordingly, on the 26th July, 1879, my patient called for Prof. Spence, in Edinburgh. Mr. Spence advised a palliative treatment, and prescribed a simple lotion of borax and glycerine. His recommendations were faithfully attended to for several months, but without any appreciable result. Trifling as the sore appeared—being entirely confined to the tip of the nipple—I felt certain that there was some malignancy about it, so I arranged for another consultation with Mr. Spence. At our meeting, which took place on Jan. 5, 1880, the breast was carefully examined. The left mammary gland was slightly larger than the other, but this was satisfactorily explained by the patient, who stated that this peculiarity existed since the time of suckling her children. The sore, upon the removal of a yellowish crust, had a reddish vascular appearance, but there was no hardness in the gland itself, nor any appearance of glandular enlargement. In these circumstances it was Prof. Spence's opinion that we were not justified in removing the mamma, but considering the intractable nature of the sore, he advised the removal of the nipple. Accordingly, two days afterwards, 8th January, I excised the entire nipple as low down as possible, after a method suggested and described to me by Mr. Spence. The wound healed quickly and satisfactorily, and in a few days my patient was convalescent. With the exception of an occasional feeling of uneasiness in the breast, perhaps aggravated by a still lingering anxiety about herself, Mrs. A. felt no annoyance till June, 1881, when she fancied the skin affection was reappearing in the cicatrix, and I was asked to look at it. There was a slight eczematous appearance in the centre of the areola, and with a magnifying glass I could see the vascular condition of the blood-vessels. I could not, however, be positive that there was any material change in the mammary gland, so I ordered a soothing lotion, and as the lady was leaving for the coast, I heard no more of her till about two months later, when she became alarmed about a marble-like tumour which she detected in the same breast. Upon examination I found not only a distinct tumour attached to the upper and inner edge of the mamma, but a glandular enlargement of the size of a pea at the margin of the axilla, together with a distinct fulness of the glands at the base of the neck. As it would be quite hopeless to eradicate the disease by operative interference, I advised my patient against this procedure, and Prof. Macleod entirely concurred.

Case 2.—About four years ago, Mrs. C. D. suffered from an irregular patch of eczema on the left breast, regarding which she asked my opinion. The disease covered an area rather larger than two half-crown pieces, and was situated on the lower surface of the mamma, but involved neither the nipple nor areola. In a few months she was completely cured by the external application of an ointment of oxide of zinc, glycerine and carbolic acid, together with the internal administration of liq. arsenicalis. Nearly a year later Mrs. C. D. again appeared at my consulting rooms seeking advice in regard to the same breast. It was not, however, *eczema* this time, but a hard nodule in the mammary gland which troubled her. I did not strongly advocate excision, and she herself appeared to be against operative interference. During the following year I saw Mrs. C. D. occasionally, and the tumour appeared to undergo very little change. Towards the end of the year 1879, she became alarmed by a slight bluish appearance which the surface of the tumour had assumed, and influenced by friends and the advice of other medical men whom she consulted, she appeared suddenly very desirous to have it removed. I assisted at the operation of removal on the 14th Feb., 1880, and on careful inspection of the tumour afterwards, none of the medical men present had any doubt of this being a true scirrhous tumour. Mrs. C. D. made a good recovery, but the disease reappeared in the following September, and she now entirely confined to bed. This patient is married and has had two children. Her mother died of consumption, aged 27 years, and her own age is 62 years.

Case 3.—E. F., unmarried, aged 42, has been under treatment for eczema of the nipple and areola for about a month. This patient has had eczema in other parts of her body for years, and the present attack seems to be true eczema. The result of the month's treatment is that she is now almost entirely free of the disease. The areola and whole surface of the nipple were affected, and the disease was very well marked.

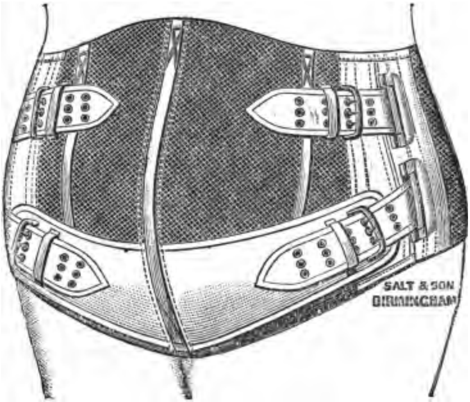
In applying to these cases the diagnostic test suggested by Sir James Paget—viz., intractability to treatment, the first only satisfies its conditions, and true to his prediction, we have in due time the formation of the secondary disease. It, in fact, conforms with every clinical and pathological observation hitherto made regarding "Paget's Disease," of which, therefore, we must consider it a typical example. Case 2 is not an affection either of the areola or nipple, nor was it intractable to treatment, and yet the termination is precisely the same as in the former, and from the history it will be seen that the earliest development of the breast tumour must have been coincident with the eczema. The question may therefore be fairly asked—Had this skin affection any casual connection with the subsequent carcinoma? Having pointed out the sequence we will dismiss the case by supposing it to be nothing more than a mere coincidence. The experience of a few weeks' treatment consigns my third case to the category of pure and simple eczema.

The fact, therefore, that all cases of eczema of the nipple, occurring at the period favourable to the development of carcinoma, are not followed by cancer of the breast, brings us face to face with the most important problem hitherto suggested in the course of this inquiry—viz., the possibility of discriminating between the malignant and the benign cases otherwise than by experimental therapeutics or long-standing duration. Everybody knows that prolonged local treatment of a malignant sore, not only becomes ineffectual in the end, but increases its activity, and lessens the chance of successfully eradicating it by early removal. The early diagnosis of "Paget's Disease" is, therefore, of paramount importance. On this points, Dr. George Thin writes as follows:—"In distinguishing, clinically, this malignant dermatitis from eczema, the chief points to be borne in mind are the well defined margin in the former, and the evidence, when the tissue is grasped between the fingers, of infiltration in the papillary layer. An eczema of the areola, that had the thickening and moist redness which have characterised some of the cases of this affection, would be so acute in its nature that the zone of inflammation would pass off gradually into the surrounding skin, would probably become widespread, and would give rise to very active symptoms. The overlapping margin forming a veritable ridge, with a sulcus behind it, which characterised another case, would of itself be sufficient to exclude the diagnosis of eczema. The history of the case may be found useful in establishing the diagnosis. In two cases which have come under my notice, there was clear evidence that the disease began in a position corresponding to the openings of the lactiferous ducts, and remained limited in that position for some time before extending over the surface of the nipple."

The clinical facts in the case of Mrs. B. A. entirely agree with these distinguishing characteristics as thus expressed by Dr. Thin. The disease when first observed was confined to the central portion of the apex of the nipple, and exuded a slight discharge which formed a yellowish crust. On removal of this crust there appeared a raw reddish sore, which looked as if it had been hollowed out in the centre. For about eight months previous to the excision of the nipple it underwent no appreciable change. After the operation the breast was quite smooth, and no trace of the nipple remained except a white linear cicatrix in the centre of the areola. The recurrence of the disease was first noticed eighteen months after the operation, and then it showed itself at the margin of the cicatrix, but now the latter is entirely obliterated. At the present time the sore is of a circular shape, and about the size of a sixpence, with an irregular margin from which small whitish skin-like processes project. Its surface has a peculiarly bright red colour, and over it I have observed occasionally whitish island-like spots of this false skin which, for the moment, might cheat one into the belief that it was in the course of healing up. One speciality in the treatment of this case, which has not hitherto been recorded, is the entire removal of the supposed eczematous disease at a comparatively early stage. Its recurrence at such a long interval, and concurrently with both the breast-tumour and the more distant glandular nodules, supports, in my opinion, the theory that these cases of so-called eczema of the nipple are really cases of duct-cancer. At any rate, it is my firm belief that in the case of Mrs. B. A. here recorded, the primary sore was from the very commencement malignant disease of the epithelium at the mouths of the galactophorous ducts, and that the supposed induced or secondary disease in the breast, stands in the same relation to it as the more distant glandular deposits. Contamination through these ducts must have progressed beyond the point of excision of the nipple, so that, notwithstanding the removal of the open sore, the morbid process continued to advance steadily in the line of the absorbents; while after adhesion of the skin in the wound had taken place there was less facility for its again reaching the surface. Histologists are not yet agreed as to the pathological nature of such tumours; clinically, however, I can determine no distinction between this tumour and what I have been taught to consider as true scirrhus of the breast. — *Glasgow Medical Journal, Nov.* 1881, *p.* 542.

90.—MESSRS. SALT & SON'S NEW ABDOMINAL BELT.

This belt, of which the annexed illustration is a fair representation, is an improvement on one designed by Messrs. Salt and Son, of Birmingham, some time ago, which was favourably noticed in the Lancet, and met with much approval on the part of the profession generally.



The improvement consists principally in the substitution of a single for a double environment of the patient's body by the semi-elastic material, and in the adoption of a mode of fastening which enables the belt to adapt itself automatically, by a compensating movement to all changes in the posture of the wearer, in a manner somewhat similar to ordinary French braces, while the principle of causing the elastic fibres to correspond in direction with the abdominal muscles is rigidly preserved.

The belt consists of two sections, anterior and posterior,—of nearly equal length; the anterior (which is composed of India rubber thread interwoven in the usual way with silk or cotton, according to quality) is much the deeper of the two, and shaped accurately to fit the configuration of the figure, and supported by a central vertical strip of whalebone, and two lateral strips which diverge from each other at a slight angle from below, upwards.

The posterior section, which, as has been stated, is shallower than its fellow, is made partly of inelastic material, a sufficiency of stout bones being introduced to afford the required dorsal support and to prevent "rucking."

The connection between the two parts of the belt when in use is unique, and forms the greatest improvement noticeable in the appliance. On each extremity of the posterior half of the belt are firmly affixed two metal loops or clasps, having on their anterior aspect small tubular rollers or pulleys. Through these loops, and resting upon the pulley, pass the ends of the straps, which originate in the front half of the belt, and after passing through the loops are reflected so as to fasten to buckles placed well to the front on the straps themselves.

The lower straps are deep and curved so as to follow the abdominal lines and afford support over the epigastric region : the upper strap is narrower and almost horizontal in direction.

The following among other advantages are claimed for these belts :—

1. That the buckles being placed well to the front can be readily fastened by the patient, a manœuvre somewhat difficult of accomplishment with ordinary belts. 2. That the straps sliding over revolving pulleys yield readily and without effort to every movement of the body. 3. That the additional power given to the traction of the fingers by causing the straps to pass over the pulleys, enables the belt to be drawn to the requisite degree of tightness with half the effort needed without this contrivance. 4. That adequate provision is made for gradual enlargement of the belt, when it is ordered to be worn during pregnancy.

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